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ORIGINAL ARTICLES.

VACCINATION.

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ITS HISTORY.

The subject of vaccination is one to which the average physician pays but little attention. He, in general, believes that the prophylactic influence of vaccination over small-pox is as well established as is the usefulness of our fire departments, but if suddenly called upon to defend such belief, he might find it exceedingly difficult to acquit himself with credit. It might be supposed that one hundred years would be a sufficient time in which to settle this question beyond peradventure, and that its discussion would long ago have ceased. But it must be remembered that the fact of the circulation of the blood was denied by a majority of the medical profession in Spain, one hundred and fifty years after Harvey proved it; that even so great a man as Lord Bacon rejected the Copernican system nearly one hundred years after its publication. It should not, therefore, seem strange if we are able to find among the ignorant and clamorous declaimers against vaccination to be found in every community a few whose scientific attainments entitle their opinions to respectful consideration. Inasmuch, therefore, as we do find that the utility of vaccination is called in question by some belonging to this latter class, I have thought that it would not be inopportune at this time to take a review of the grounds upon which our opinions are based.

The origin of small-pox is veiled in the impenetrable mists of the past; where and when or how the first case appeared, no one will ever know, but it is thought that it was first recognized as a distinct disease in Asia, possibly in China, where references are made in writings of 1122 B. C. to a disease presenting the symptoms of small-pox. There is, however, no reason to doubt but that the disease had spread death in its trail for untold centuries prior to this, for the most ancient Sanscrit literatures indicate that fatal epidemics of unknown origin and name were prevalent in India 5,000 years ago, and devastated Persia before the days of Zoroaster. It was first recognized in the sixth century, when it was described by Ahron, an Egyptian physician. Rhazes, an Arabian physician of Bagdad, about 910 A. D., was the first to write specifically of small-pox and differentiate it from measles. From this time, tenth century, to the beginning of the present, the disease was prevalent in Europe to such an extent that it was universally regarded as the greatest enemy of the human race.

Malthus, writing at the beginning of this century says that "the small-pox may be considered the most prevalent and fatal epidemic in Europe." La Condamine estimated that one in every ten of the entire population died of it, while Jurin's investigations led him to place the proportion at one in fourteen.

Curschman (Ziemssen's cyclopædia), says that it has been estimated that from 7 to 12 per cent. of all deaths were due to variola. During the conquest of Mexico by Cortez, small-pox made its appearance among the natives, producing remarkably fatal results. Prescott says that it swept over the country like a fire over the prairies, smiting down prince and peasant alike, so that they "perished in heaps like cattle stricken with the Murrain." "So great was the number of those who died of this disease that there was no possibility of burying them, and in Mexico the dead were thrown into the canals, then filled with water, until the air was poisoned with the stench of putrid bodies." (Prescott, Conquest of Mexico, vol. ii. Note p. 420.) In 1717 a Greek slave taught the art of inoculation to a woman, Lady Montagu, through whose efforts its practice was introduced into England in 1721, and thence to the other countries of Europe. The practice of inoculation became very general throughout Europe, and resulted in a disease much milder than small-pox when contracted by contagion or infection. Still there was a considerable mortality following the operation, and moreover the inoculated disease was capable of spreading by the usual means. It thus resulted that small-pox became much more widespread than it might otherwise have been and it may be doubted whether the aggregate mortality from the disease was decreased by the practice. In England the operation of inoculation was prohibited by law in 1840.

Medical science does not disdain to receive information from any source, however mean. Thus it has transpired that valuable hints have been received from Jesuit priests, barbers, itinerant quacks, slaves, Indians, and even the wild bison of our western plains has contributed to our surgical knowledge. So in this instance, Dr. Jenner was led to his studies by the traditions of the cowherds and dairymaids. During the last half of the eighteenth century it was matter of common comment among the farmers of certain dairy districts in England, that those persons who had suffered from cow-pox did not contract small-pox. Mr. Nash, writing in 1781, said that he had never heard of any one having the small-pox who had ever had the cow-pox. He had inoculated sixty persons reported to have had cow-pox; of these forty were unsuccessful and twenty successful. He had reason, however, to believe that these had not really had cow-pox. Mr. Rolph had met with similar experience, and his partner, Mr. Grove, who had been an extensive inoculator, frequently having two hundred or three hundred patients at one time, by his experience with many scores of subjects who had previously labored under the cow-pox, established

their insusceptibility of the small-pox, either by inoculation or effluvia. A farmer by the name of Nash, being impressed with the traditional belief, was courageous enough to act upon it, and intentionally inoculate himself with the disease. Benjamin Jesty, a farmer of Downshay, on the isle of Purbeck, having had the casual cow-pox himself, vaccinated his wife and two sons, Robert and Benjamin, in 1774. They were all afterwards inoculated with small-pox virus and exposed to the disease by contagion, but without effect. In 1805, Jesty visited London at the invitation of the Original Vaccine Pock Institution, and there at the age of seventy years publicly submitted to the operation of inoculation with small-pox virus and afterwards with cow-pox virus, in both instances without developments. Mrs. Catherine Wilkins Titchenor, of Wiltshire, voluntarily submitted to inoculation for small-pox at the hands of Dr. Archer, in 1781, to demonstrate to her brother that she, having had cow-pox, was proof against small-pox, and the operation sustained her claim. It is said that a school teacher in Holstein in 1791, inoculated three boys with the virus of cow-pox but no details of the cases are on record.

But traditions of this sort were by no means confined to the dairy districts of England. On the contrary (Good's Study of Medicine, vol. i, p. 612; 6th Amer. Ed.) the same belief prevailed in the country districts in France, Spain, Norway and Germany, and Humboldt in his work on New Spain adduces evidence which would indicate that the inhabitants of the Andes were long in possession of similar knowledge. It will thus be seen that observations tending in the same direction were made at numerous and widely separated places.

Although observations and traditions of this kind were thus widespread in the latter years of the last century, this fact should detract nothing from the credit due to Dr. Jenner as the first to introduce the fact to the active attention of the scientific world. Edward Jenner, who had been a pupil in London of the great Scotch Pathologist, John Hunter, was an humble practitioner of medicine at Berkeley, in the county of Gloucester, England. He there became acquainted with the traditions current among the cowherds and dairymaids, and determined to investigate the subject. He undertook to ascertain, if possible, the origin of the disease in the cow, and believed that he had done so, but this subject will be adverted to later on. Having observed a large number of cases of the casual cow-pox and noted their subsequent immunity from small-pox, the next important step to be taken in this, to him, exceedingly interesting investigation, was to ascertain whether the cow-pox could be transmitted by inoculation from one human being to another, and if it could be thus transmitted whether it retained it protecting power. It was a long time before an opportunity presented itself to him of making this experiment, but at length it came, and on May 14, 1796, he vaccinated his first subject with matter taken from the hand of one Sarah Nelmes.

He watched this case with great care and anxiety, and first detailed the history of the case to his friend Gardner, to whom he wrote as follows on July 19, 1796: "A boy of the name of Phipps was inoculated in the arm from a pustule on the hand of a young woman who was infected by her master's cows. Having never seen the disease but in its casual way before, that is, when communicated from the cow to the hand of the milker, I was astonished at the close resemblence of the pustules, in some of their stages, to the variolous pustules. But now listen to the most delightful part of my story. The boy has since been inoculated for the small-pox, which as I ventured to predict, produced no effect. I shall now pursue my experiments with redoubled ardor."

During the year he encountered other cases of failure of small-pox inoculation in those who had had cow-pox and believed that he now had materials for a paper which he was ambitious to have appear in "The Transactions of the Royal Society."

He at once began writing his paper on "The Causes and Effects of Variolæ Vaccinæ." This inquiry, which was somewhat hurriedly written, was presented to the Royal Society but was rejected. In this paper Jenner detailed, besides the case of the boy Phipps, the following ten cases of casual small-pox:

	Had Cow-pox.	Inoculated for Small-pox.	Result.	Exposed to Small-pox by Infection.	Result.
1.	1770	1795	Unsuc-	In family.	No indis-
2.	1765	1792	cessful.	Nursed children.	position.
3.	At age 9 years.	At age of 62.	"	Not stated.	
4.	1760	1791	"	Repeatedly employed as small-pox nurse.	"
5.	When very young.	1778	"	Visited a relative sick with small-pox.	"
6.	1796	1797	"	Not stated.	
7.	1796	1797	"	" "	
8.	1759	1792	"	" "	
9.	1780	1795	"	Assisted nursing small-pox cases.	"
10.	1782	Some yrs. later.	"	Repeatedly exposed.	"

And the conclusion which he reached was "but what renders the cow-pox virus so extremely singular is that the person who has been thus effected is forever after secure from the infection of small-pox; neither exposure to the variolous effluvia nor the insertion of the matter into the skin producing this distemper."

Two years later, that is in 1798, he rewrote his Inquiry, adding considerable new material, and had the same published. This publication produced a great stir in the medical profession. No sooner did Jenner's conclusions become public property than opposition to them began to appear on every hand. Dr. Ingenhousz, one of the most prominent physicians in London, was one of the first to question the utility of the new operation. Mr. John Birch, surgeon to His Royal Highness, the Prince of Wales, was another who particularly distinguished himself as an opponent of vaccination. He published in 1806 an extensive document in which he set forth his reasons for objecting to the operation. This was in answer to "The Report of the Jennerian Society." He was a surgeon and an extensive inoculator, and all through his paper the two operations are contrasted to the advantage of inoculation. Moreover, he was specially exasperated because when the committee of the House of Commons were investigating the subject they did not consult the College of Surgeons, whereas, they did consult the College of Physicians and the apothecaries. He wrote at a time when the balance of opinion among the masses was decidedly in favor of the new procedure, but so thoroughly convinced was he of its inefficacy that the language of his paper is not specially conspicuous for temperateness, and he ventured the prediction that the popular opinion favorable to vaccination, then in the ascendant, would very soon "vanish into thin air."

In 1799 cow-pox made its appearance in the dairies of London, a circumstance which greatly aided the

experiment. Drs. Pearson and Woodville, who were extensive inoculators in London, procured supplies of this virus and immediately began vaccinating on a large scale. The operations of these gentlemen constituted a very important epoch in the history of vaccination. They reported about 500 cases with one death in a very short time, and the uniform immunity of these persons from small-pox did a great deal to establish the operation on a firm basis. In March, 1799, Dr. Pearson sent supplies of this virus to numerous practitioners in England, requesting them to use and report results. He soon afterwards sent supplies of vaccine virus to Paris, Berlin, Vienna, Geneva, also to Hanover, Portugal and America. In Germany the first vaccination was performed by Hugo von Wreden, in Hanover, then under English rule. In 1799, Surgeon Christian Frederick Stromeyer, who was educated in England, began vaccinating and induced the court physician, Dr. Georg Fr. Ballhorn, to translate Jenner's work into German. The next year (1800) these gentlemen were able to report on 1,000 cases of vaccination.

Ferro, in 1799, was the first to perform the operation in Austria, using one of his own children as a subject. Dr. Jean De Carron, of Geneva, then in Vienna, also operated in the same year.

In the year 1800, Heim established a Jennerian society in the city of Berlin, and did an extensive business. In France vaccination was promoted by Pinel and Thouret, of Paris, in Holland by Dr. Ludwig H. J. Demanet, of Ghent, in Italy by Aloysio Sacco, professor of medicine and surgery in Milan, in Denmark by Heinrich Callisen, in Spain by Amar, Gil and Salva. Dr. Benjamin Waterhouse, of Boston, professor of practice of medicine in Harvard college, vaccinated four of his children in July, 1800, having procured his virus from Haygarth, of Bath, England. Dr. Waterhouse has generally been conceded to have been the first to perform the operation in America, but at the meeting of the American Medical Association in 1874 (Transactions, vol. xxv, p. 59) Dr. Brodie, of Detroit, presented to the society a pamphlet showing that Dr. Valentine Seaman was the first to introduce the operation of vaccination into this country. The pamphlet was accompanied by the lancet with which the first vaccination was made. During the summer of 1800 Dr. Crawford, of Baltimore, performed the operation with virus received from Dr. J. Ring, of London.

The clergy rendered effective service in inducing the people to be vaccinated; this was particularly the case in Germany. The Dowager Empress of Russia became a patron of vaccination, and thereby greatly aided its introduction into that country. In 1800, Joseph Marshall and John Walker made a tour teaching and practicing vaccination; they stopped at Gibraltar, Minorca, Malta, Palermo, and Naples.

Marshall wrote to Jenner that at Palermo it was "not unusual to see, in the mornings of the public inoculation at the hospital, a procession of men, women and children, conducted through the streets by a priest carrying a cross, come to be inoculated."

Dr. Balmis was largely instrumental in establishing the practice in various parts of Spain as well as Spanish America, and in Spain's Asiatic dominions, also in the Canary Islands, Porto Rico, Caracas, Lima, Chili, Charcas, Havana, Yucatan, Gautemala, Acapulco, the Philippine Islands, Macao, Canton and St. Helena. The British Ambassador took vaccine virus from the National Vaccine Establishment to Persia, and from Vienna De Carro sent supplies to Constantinople and thence to Bombay.

The old practice of small-pox inoculation, though about extinct in civilized countries, has been continued even to the present time in some parts of the world.

This is the case in Burmah, where it has held its own until quite recently, although it is said that in certain parts of the country the mortality from the inoculated disease has averaged for long periods 8 per cent. When the English occupied this country a few years ago they endeavored to substitute the operation of vaccination for that of inoculation, but not with entire success. In the little village of Khramaung (*Lancet*, vol. i, 1891, p. 625) a number of persons were last year vaccinated. Their faith in the efficacy of the operation was, however, very limited, and they accordingly sent for the Sayas to inoculate them with small-pox virus. This operation was unsuccessful in every case, notwithstanding the fact that in each instance it was repeated three or four times. These were regarded as in the nature of test cases and the facts appealed to the understanding of the villagers, ruined the reputation of the Sayas, and "induced the headmen for a considerable distance around Sandoway to make earnest appeals to the civil surgeon to send vaccinators to their villages."

A very interesting item in the early history of vaccination is afforded in the experience of six black children who were vaccinated in 1804, and who furnished the stock from which 5,000 of the inhabitants of the island of Réunion were vaccinated. These children were shipped on board the vessel "Jeune Caroline," bound for one of the Seychelles Islands to perform quarantine for small-pox. These children were exposed on this vessel constantly, for a period of three months, to the disease, by infection and contagion. They slept under the same blankets with the sick, and in contact with their pustules, and they were inoculated on the arms with matter taken from these pustules, yet nevertheless they remained during all that time in a state of perfect health. There were on board this vessel at that time twenty blacks suffering from confluent small-pox, of whom six died. There were also between twenty and twenty-five others with dry crusts all over their bodies, and of whom seven died—and all these were packed into a small space between the decks of this vessel. The immunity from the disease enjoyed by these children is a striking example of the prophylactic power of vaccination, for they were submitted to the crucial test of inoculation and repeated and prolonged exposure to the effluvia of the disease.

It thus resulted that within a very few years after the publication of Jenner's "Inquiry" in 1798, vaccination was known and practiced throughout the civilized world. Though violently assailed at the very first, being denounced as not only foolish, but unnatural and impious, to engraft the disease of a brute upon a Christian, nevertheless the popular belief in its prophylactic power was soon firmly established. And the evidence soon accumulated by physicians from all countries soon established its position in the medical world, although no satisfactory solution was found to the question as to the how or the why.

The mortality resulting from small-pox inoculation was always considerable; in fact, so large as always to be a more or less serious barrier against its

universal practice. When, therefore, the operation of vaccination was proposed as a substitute, and the people were assured that the disease could not by its effluvia give rise to either cow-pox or small-pox, they were prepared to tolerate a certain percentage of mortality resulting therefrom, though Jenner at first thought that death could never result directly from the operation. The early history of vaccination is marked by numerous experiments, inoculating individuals with matter from the sore heels of horses, etc., and there is no doubt whatever that some of these cases resulted fatally from septic infection, and the death was attributed to the action of vaccine virus.

Vaccination having established itself on firm grounds, and it being recognized that people, with the very best intentions and liveliest belief in the prophylactic power of the operation, will quite generally delay its performance until danger threatens, the subject early became matter for legislative action. Compulsory vaccination was first adopted in Germany, in Bavaria and the Grand Duchy of Hesse in 1807, in Baden in 1815, in Würtemburg in 1818, and in 1829 revaccination was required of recruits in the army. Compulsory laws were also passed by Denmark in 1810, Sweden 1814, Prussia 1835, Roumania 1874, Hungary 1876, Servia 1881. In ten of the twenty-two Swiss cantons it is also compulsory. The different laws in force in the German States and Principalities were merged in the general law of 1874 of the German Empire. In England a vaccination law was passed in 1857, in South Australia in 1872, Victoria 1874, Western Australia 1878, Tasmania 1882, Calcutta 1880, and since that in eighty other towns of Bengal.

In Egypt and its dependencies vaccination was made compulsory on December 17, 1890. The law requiring that the operation be performed by the end of the third month, except in case of sickness, when it may be postponed upon a physician's certificate.

In Manitoba a vaccination law was enacted and published in 1885. It is there provided that the poor may be vaccinated at public expense; that the operation must be performed in the first three months of life; that a certificate must be obtained of successful vaccination, or a certificate for delay or insusceptibility. Penalties and cumulative penalties are imposed for non-compliance with the law within the required period. Teachers are required to enforce the law, and are liable to penalties for neglecting to do so. In Japan vaccination was practiced to a very limited degree twenty-five years ago, at which time a large proportion of the population were pock-marked, but in 1874, and again in 1885, compulsory laws were enacted which require that not only those who have been unsuccessfully, but those successfully vaccinated, shall be revaccinated within a period of between five and seven years after the first operation. It is recommended that in case of epidemics of small-pox, all shall be revaccinated, no matter how recently the operation had been successfully performed.

A movement is now on foot to make vaccination compulsory in China. In Italy a law was recently passed, which went into effect January 1, 1892, which requires that every child be vaccinated within the first six months of life. Heretofore the only efforts in this direction in Italy have been as regards soldiers and other persons in the employ of the State.

Revaccination was made obligatory in Denmark in 1871, in Roumania in 1872. In Belgium there is no compulsory law, and no disability or prohibition laid upon the unvaccinated.

In France, Spain, Portugal, Norway, Austria and Turkey, there are no compulsory laws, except as regards soldiers and other State employés, but unvaccinated children, in many instances, are refused admission to the public schools and some of the private schools.

Although there have been compulsory laws on the statute books of some countries for many years, in one instance since 1807, it has happened, as we often see with other laws nearer home, that they have been executed with a very lax hand—in fact, have fallen into "desuetude," if not "innocuous," and the result has been that only a comparatively small proportion of the people have taken advantage of the protection against small-pox afforded by the operation. This fact was abundantly demonstrated in 1871–1873, when a very destructive epidemic of small-pox swept over Europe. Since that time the laws have been very vigorously enforced in the German Empire and in Great Britain, though even in these countries there are many who neglect to comply with the law now.

In this country we have no compulsory vaccination laws, but an effort in this direction is made in many States by the enactment of compulsory education laws, and excluding unvaccinated children from the schools.

It is not alone those who do not believe in the prophylactic influence of vaccination who neglect its performance, but even those who are most alive to its importance will neglect it until small-pox appears in their neighborhood, when there is a very general demand for the operation. So true is this, that when a small-pox scare occurs, the demands for vaccine virus are so great that the propagators are utterly unable to supply them, and every animal is made to yield the largest possible quantity of virus. This was the case in 1882, and again in 1885. It thus happens that, at such times, very much of the lymph used is spurious, producing an atypical vesicle, leaving an atypical scar, and affording little, if any, protection against small-pox.

When we stop to consider what a large number of people are vaccinated who do not have typical vesicles or good scars, it is easily understood how the mortality is so great as it is among the vaccinated. Nor is this the fault of the physician, for my experience and that of many of my professional friends is that the people, in a very large proportion of cases, never report to the vaccinator after the operation, so that he is entirely ignorant as to its results, and they, presuming that it has been successful, feel that they are protected against small-pox, and often expose themselves to the disease, only to contract it, and thereafter to advertise themselves as living examples of the futility of the operation.

ITS PATHOLOGY.

In the first paper published by Dr. Jenner on variolæ vaccinæ, he did not neglect a consideration of the subject of its pathology. We find upon the first page of the "Inquiry": "In this Dairy Country a great many Cows are kept, and the office of milking is performed indiscriminately by Men and Maid Servants. One of the former having been appointed to apply dressings to the heels of a horse affected with *the Grease*, and not paying due attention to cleanliness, incautiously bears his part in milking the Cows, with

some particles of the infectious matter adhering to his fingers. When this is the case, it commonly happens that a disease is communicated to the Cows, and from the Cows to the Dairy-maids, which spreads throughout the farm until most of the cattle and domestics feel its unpleasant consequences. This disease has obtained the name of the Cow Pox."

He performed some experiments with a view to proving the truth of this supposition. Thomas Virgoe, having been infected from a mare's heel, furnished matter from a pustule on his hand, with which Jenner inoculated a boy named John Baker, 5 years old, in March, 1798. This boy died soon afterwards, possibly from the septic effects of the inoculation. Jenner later continued these experiments, inoculating a cow with matter from a horse's sore heel; with virus thus obtained he continued arm to arm vaccination for five or six generations, when the stock was lost. Several of the subjects thus vaccinated were afterwards submitted to small-pox inoculation, but without success. Other experimenters soon continued the investigations initiated by Jenner. Conspicuous among these was Dr. John G. Loy, who in the year 1801 published "An Account of some Experiments on the Origin of Cow Pox." Proceeding on the horse grease theory, he inoculated cows with matter from the sore heels of horses, reporting which experiments he said: "In the manner of inoculation we have imitated exactly the process by which Dr. Jenner supposes the genuine Cow Pox to be produced. But it was not till after several trials that I was convinced the infection of the Grease could be made to operate upon the Cow without having been previously made to pass the action of the human system; for I made several unsuccessful attempts to produce any appearance of the Cow Pox by the application of Grease matter as obtained directly from the heels of the horse. Matter taken from three different horses in Grease, and at different times of the disease, did not produce, when inserted into the teats or udder of the cow, the least appearance of the Cow Pox."

At length, however, he met with a horse from whose heels he procured matter, in a more limpid state, with which he inoculated a cow, thereby producing a vesicle "containing a large quantity of watery fluid of purplish tinge." With this material he inoculated a child, producing a vesicle. This child was afterwards unsuccessfully inoculated with small-pox virus. From these experiments Dr. Loy concluded that there must be two kinds of horse grease, "differing from each other in the power of giving disease to the human or brute animal. . . . The horses that communicated the infection to their dressers were affected with a general as well as topical disease." In fact, at that time the diseases of the horse were not as well understood as at present, and thus two diseases were then confounded. The grease is a local disease of the horse's heel, being an inflammation of the sebaceous glands in and about this locality. It may be caused by sudden changes from heat to cold, or *vice versa*, and hence may result from washing the legs with hot water, and allowing them to dry by evaporation. It may also result from neglecting to dry the parts after unusual exertion. When grease becomes chronic, the affected part is infested by numerous acari, the *sarcoptes hippopodus*. On the other hand, horse pox is a disease affecting the general system, there is fever and evident indisposition, which is ameliorated when the generalized skin eruption appears, and this eruption is usually plentiful about the horse's heels. This disease, then, is the *grease* to which Jenner attributed the origin of the cow-pox. This theory of the origin of cow-pox was never very generally received by the medical profession, but recent investigations of Fleming in England, and Prof. Peuch of Toulon, France, appear to lend strength to this view.

The most natural theory would be, that inasmuch as vaccination confers immunity from a subsequent attack of small-pox, it must be a variolous affection of the cow, or as Jenner called it, variolæ vaccinæ; that is, cow small-pox.

Experiments were early performed with a view to determining the tenability of this theory. In 1828 the cow was successfully variolated by several physicians in Egypt, and children were vaccinated with virus from this source. Since that time very many repetitions of these experiments have been made, by Sunderland, Ceeley, Badcock and others. In many instances, to be sure, the persons vaccinated with virus taken from the variolated cow have had small-pox as a result. This was the case in a number of instances in the Confederate Army during the late war. A recent experiment of this kind was performed by a Dr. Meyer, of Monee, Ill., in 1880 (*Chicago Medical Journal and Examiner*, 1881, p. 481). Having variolated a heifer, he used the resulting virus in vaccinating a number of patients, most of whom contracted a mild form of small-pox as a consequence, but a few developed a more severe form of the disease. The animus thereby created in the village against the doctor, was such as to induce him to "fold his tent like the Arab, and as silently steal away." These experiments, and hundreds of others of a similar character, have been received by a large portion of the medical profession as indicating conclusively that cow-pox is, in reality, cow small-pox, a disease mitigated by transmission through the cow, and hence its prophylactic influence over small-pox. This is the doctrine taught in the majority of our text-books at the present time, notwithstanding the conclusion of the Lyons Commission that they are two distinct diseases.

Mr. John Birch, already referred to, did not believe that cow-pox was conveyed to the cow from the heels of the horse, but suggested that it might be communicated to the cow by the hands of a milker suffering from the itch. Auzias-Turenne, in a communication to the Academy of Medicine, Paris, in 1865, called attention to the analogy between cow-pox and syphilis; that there is in early removes from the cow, the formation of a papule, a vesicle, an ulcer with surrounding induration, enlargement of the lymphatic glands, and secondary eruptions or vaccinides.

There are those, even, who claim that the cow originally contracted the disease from the hands of milkers suffering from syphilis, notwithstanding the fact that the cow has been shown to be unsusceptible to this disease. This claim has, I believe, never been advanced by any one of scientific standing.

Prof. Crookshank, who is at present probably the most considerable opponent of vaccination, arrives at conclusions which may be briefly stated as follows: 1. Small-pox and cow-pox are radically dissimilar diseases. 2. It is not possible to protect from one disease by the artificial induction of a radically dissimilar disease. 3. Therefore, cow-pox does not protect against small-pox.

A large portion of the first volume of his recent

work on the "History and Pathology of Vaccination" is taken up with material from which he deduces his conclusion that cow-pox and small-pox are radically dissimilar diseases. This material is as follows: The experience of inoculators was that matter from a case of confluent small-pox was very likely to cause the disease in a severe form, while from a mild case a still milder form of the disease would result. Adams, an extensive inoculator, acting on this knowledge, succeeded, by inoculating from very mild cases and continuing the process from arm to arm, in producing an inoculated disease so mild as to raise the suspicion that the virus of cow-pox had been used, there being no secondary eruption and the disease being no longer infectious. Guillon later also succeeded in producing vesicles with all the characteristics of a vaccine vesicle by cultivation from small-pox. Dr. Thiele, of Kasan, in 1839, met with the same results.

In 1828, Dr. McMichael reported that several Egyptian physicians had succeeded in variolating cows, and from that source had raised a stock of vaccine lymph. In 1830, Dr. Sonderland, of Barmen, variolated several cows by infection, and with matter thus produced succeeded in producing the typical vaccine disease. In 1839, Ceeley inoculated a large number of cows with variolous matter, and in several instances with success; with virus thus secured he began vaccinating and produced typical vaccine vesicles, continuing the stock. Badcock, of Brighton, in 1840, also succeeded in variolating the cow, and from the pustules thus resulting derived a stock of vaccine matter which produced typical results. Other experiments of this kind were not as happy, many cases of small-pox resulting from vaccinations with lymph from the variolated cow. Cattle plague is a disease which is closely analogous to small-pox in man, and is thus described: "The disease among horned cattle is an eruptive fever of the variolous kind; it bears all the characteristic symptoms, crisis and event of the small-pox, and whether received by contagion or by inoculation, has the same appearance, stages, and determination, except more favorable by inoculation, and with this distinctive and decisive property, that a beast once having had the sickness, naturally or artificially, never has it a second time."

It is an exceedingly fatal disease among animals, and, as Dr. Murchison has shown, is closely analogous to small-pox in man.

Mr. Macpherson, Superintendent of Vaccination at Moorshedabad, India, in 1832 selected matter from a mild case of this disease and vaccinated a number of children, in one case producing a typical vaccine vesicle. The symptomatic fever in this case was rather more severe than usual. From this child others were vaccinated with success and a new stock of vaccine lymph thus secured. In order to assure himself that "true cow-pox" had been introduced, a number of these children were afterward inoculated with small-pox virus and exposed to it by infection and contagion, but without taking the disease. The stock thus raised was extensively used in India.

Mr. Furnell, in Assam, repeated the experiment of Macpherson in 1834. With the assistance of Mr. Brown he inoculated several native children from a cow recovering from cattle plague, and then propagated the virus from arm to arm. An officer's baby was vaccinated in November with this virus, but developed a disease in all respects similar to small-pox. Mr. Furnell vaccinated his own child from a child vaccinated on the same day on which the patient was vaccinated which furnished virus for the officer's baby and from the same source; his child developed a disease in all respects similar to small-pox in its confluent form and died. Other similar cases occurred.

Mr. Hancock, of Uxbridge, England, in December, 1865, in conducting an autopsy of a bullock recently dead of cattle plague wounded his hand, and there developed at the site of the wound a typical cow-pox vesicle, although he had been successfully vaccinated in infancy. Sacco and Dr. Legni in Italy in 1802 encountered sheep-pox, and with virus from that source vaccinated a number of children, producing vesicles which in every way advanced as is usual with cow-pox. Legni propagated this virus from arm to arm for years, and saw the children so vaccinated exempt from small-pox during a three years' visitation of the disease at Pesaro.

Prof. Heydeck, in Spain, experimented in 1803 with goat-pox, and speaks of vaccinating the children in a foundling house with virus from this source, which "did its effects." There are no details of these experiments. Jenner, Sacco, Loy, and others succeeded in raising a stock of vaccine virus from horse-pox, or, as Jenner thought, horse grease. Virus from this source was extensively used for a number of years prior to Jenner's death, and "proof was obtained of the patients being duly protected." Equine virus has not been used in England since Jenner's time, unless it has been handed down since then by arm to arm vaccination.

In France, however, at present equine virus is quite extensively used, and M. Layet, of Bordeaux, has twice renewed his stock of virus from the horse, and he testifies that it has given great satisfaction.

And this is the evidence upon which we are asked to conclude that cow-pox and small-pox are radically dissimilar diseases, and we are, therefore, to abandon vaccination! It is impossible to read Crookshank's book without being struck with its partisan tone. Everything unfavorable to the operation is given great prominence and emphasis. Unfairness and even dishonesty are imputed, not only to Jenner, but to other advocates of vaccination; and he entirely neglects to inquire into its prophylactic power, as shown by actual experience for the past hundred years. According to him Jenner was a man of no special calibre; he made no discovery, performed no new experiment, except, possibly, inoculation from horse-pox, and his "masterly induction" was not only not "masterly," but absolutely untenable.

It would appear that the legitimate inference from the facts so laboriously collected by Crookshank is altogether different from that which he has drawn. He has cited testimony, already detailed, which shows that a typical vaccine vesicle can, by management, be produced from different sources, namely—small-pox, cow-pox, horse-pox, sheep-pox, goat-pox, cattle plague, and from the variolated cow, as well as by retro-vaccination, and that a great deal of evidence has been brought forward to prove that they all afford protection against small-pox both by inoculation and infection. Some years ago the cattle plague, or rinderpest, appeared among the cattle in certain portions of England, and Murchison theorized that if this disease were the analogue of human small-

pox, vaccination of cattle with vaccine virus ought to protect them against infection. He therefore made some experiments in order to ascertain if this were true. (*Lancet*, 1866; page 301 and 356.) Cattle were vaccinated in thirteen different herds where rinderpest had affected some of the animals. The vaccinated cattle either escaped the disease or suffered from it in a mild form, the protection afforded by the operation being, however, temporary.

M. Haccius, Director of the Vaccinal Institute of Lancy, at Geneva, and Eternod, Professor of Histology in the Geneva School (*Boston Med. and Surg. Journal*, January 22, 1891, page 101) have recently published the results of their investigations as to the identity or duality of small-pox and cow-pox. They inoculated a heifer with virus from a case of confluent small-pox, and propagated the resulting stock to the fourteenth generation, producing effects absolutely identical with those of vaccine virus. They stated that virus from the fourth generation has produced typical effects used for the purposes of vaccination. They have inoculated with small-pox virus a large number of animals and always with success.

In their operations they pursued a different plan from that followed by former investigators in this field, that is, they simply denuded a limited surface by scraping with a piece of glass or other instrument, merely drawing the faintest tinge of blood, the method ordinarily pursued now in vaccinating, then with a spatula rubbed in the virus. Ceeley, Baron, Thiele, Voigt and others inserted the virus deeply into incisions made through the skin and very often by this method failed to infect the animals. All the animals thus variolated by Haccius and Eternod were afterwards vaccinated, but they proved refractory. Their conclusion, therefore, was that vaccinia and variola are identical, the former being a modified and attenuated form of the latter. The conclusions reached by the Lyons Commission about forty years ago were (small-pox and cow-pox, Auzias-Turenne, Crookshank, Vol. ii, p. 546): 1. "Cow-pox and small-pox can be inoculated in bovines and equines; the first energetically, the second feebly; 2. Whatever may be their successive transmissions direct or crossed, these two diseases breed true on every soil; 3. The protection from one by the other is assured in every case."

Chauveau, who was one of the most prominent members of this commission, has reviewed the report of Haccius and Eternod, and has repeated with their lymph some of their experiments (*Boston Med. and Surg. Jour.*, Dec. 3, 1891, p. 607). He fails to corroborate the conclusions reached by these gentlemen. The lymph used by Chauveau was from the eighth generation and he found that in every case the animal experimented on showed an eruption which had special characters distinguishing it from vaccinia. The animal thus inoculated could not afterwards be successfully vaccinated. He therefore substantially maintains the conclusions of the Lyonese Commission, believing that while they must be regarded as distinct diseases, they proceeded from the same origin, a fact which had not yet been established. As to preventive inoculations he formulates three methods as follows: 1. Fabrication of prophylactic substances by pathogenic agents outside the organism to be rendered immune, and the introduction into this organism of the said substances in sufficient quantity to confer immunity, these substances having been, by the proper manipulations, freed from the virulent elements properly so-called and rendered inoffensive. 2. Fabrication in the organism to be protected of prophylactic substances by pathogenic agents, with the germs of which inoculation has been made under conditions which insure the benignity of their infectious effects. 3. Fabrication of the vaccinal substance by a virus very like the virus against which immunity is sought, but belonging to another species." In the third category Chauveau would place the vaccinal substance fabricated from the microörganism of chicken cholera which possesses the property of conferring immunity against charbon, and here also belongs the virus which is used as a prophylactic against small-pox.

Bacteriological research in the class of variolous diseases of men and animals is still in its young infancy, nevertheless, Coze, Feltz, Lugenbühl, Weigert, Strauss, Garrè and Wolff assert (Rohè, Textbook of Hygiene, 2nd Ed., p. 302) that they have found specific microörganisms in the contents of variolous pustules, in the blood of patients suffering from the disease, and in vaccine lymph.

Hallier (Transactions Am. Med. Ass'n., 1872, p. 230) made cultivations from both vaccine and variolous lymph on such media as lemons, white of egg, and starch moistened with a solution of phosphate of soda. A number of fungi developed and Hallier thought he had traced the origin of both the vaccine disease and variola to the same fungus and thus explained the prophylactic influence of one over the other. Bender, in 1859, made microscopic examinations of matter from small-pox pustules in which he found spores.

Salisbury repeated the experiments of Hallier and traced the origin of these diseases to a plant which he called *ios Variolosa Vacciola*. This plant presented two phases, the fungoid *ios Variolosa*, and the algoid *ios Vacciola*. He thought that in variola the vegetation developed in all its phases, and that in vaccinia only the algoid phase was reached, the *ios vacciola*. These investigations were conducted before the science of bacteriology existed and are only interesting as a prelude to subsequent studies. Since Koch demonstrated that certain germs suspected to be pathogenic could be cultivated in media outside the body, attempts have been made to cultivate the microörganisms of vaccinia and variola, for a number of bacteriologists have found such microphytes which in their physical aspects are identical in the two diseases, and which it was once thought Chauveau had proven to be the specific cause of small-pox.

Quist, of Helsingfors, Finland (Ref. Handb'k of Med. Sciences, Vol. vii, p. 551), succeeded in cultivating vaccine virus on artificial media, the specific element he thought to be a spherical micrococcus of small size, and a bacillus which he regarded as the adult form of the same.

Garrè in 1887 reported the results of his experiments. He succeeded in producing a culture of the cocci in culture ovens or agar-agar and blood serum, but vaccinations with these cultures failed. Some years ago the Worshipful Company of Grocers of London offered a prize of £1000 for the solution of the problem which was stated by them as follows: "To discover a method by which the vaccine contagion may be cultivated apart from the animal body in some medium or media not otherwise zymotic; the method to be such that the contagion may, by means

of it, be multiplied to an indefinite extent in successive generations, and that the product, after any number of such generations shall (so far as can within the time be tested) prove itself of identical potency with standard vaccine lymph."

So far as I am aware this prize has never been claimed, but it stimulated anew investigations in this direction. In suitable media, as in the arm of an unvaccinated person, the microörganisms of vaccinia multiply with great rapidity. The cell increases in length, becomes constricted about the middle resembling somewhat a dumb-bell, this constriction increasing until the cell is cut in two. Dr. Dougall experimented with several artificial media after carefully examining the natural medium vaccine lymph. This he found to be free from microörganisms other than the vaccine cells,and he therefore concluded that the artificial medium must be aseptic. He performed one hundred and eighteen experiments during a period of three years, using as media, egg albumin, veal and beef broths, blood serum, gelatine, meat peptone, decoction of barley, extract of malt, etc., all of which were rendered either slightly alkaline or neutral. Upon using these cultures for purposes of vaccination he failed in every instance to produce the vaccine disease.

In view of all these facts and experiments it is difficult to understand how Crookshank could arrive at the conclusion that cow-pox and small-pox are radically dissimilar diseases. On the contrary it would seem more rational to conclude that there is *something* in common between all the diseases mentioned, from which vaccine material has been derived, namely, small-pox, cow-pox, horse-pox, sheep-pox and cattle plague. That though small-pox and cow-pox may be marked by many points of dissimilarity, it is entirely within the range of probability that they sprang from the same original source. If the specific active cause of these diseases is a microörganism, as analogy would lead us to think probable, to say the least, it is certainly conceivable that originally it may have been one and the same; that during the long period of time in which the diseases have existed on this planet, this germ may have become so modified in its pathogenic properties by its environments as to give rise to diseases which in many respects are dissimilar. The points of dissimilarity between variola hæmorrhagia, and a mild varioloid in which there are but few or a single pustule, are certainly very striking and yet no one for a moment doubts that they are the same disease. The dissimilarity between these two forms of the same disease is not less striking than that between a mild varioloid and the vaccine disease,and is certainly much greater than that between the inoculated small-pox, as observed by Adams, Guillon and Thiele, and vaccinia, where the diseases were entirely indistinguishable.

It is by no means impossible that bacteriological studies of these diseases of men and animals which present so many points of dissimilarity, will be found to have developed their distinguishing characteristics by a process of evolution, and it is possible even that the circumstances which have resulted in their differentiation may be pointed out. Man himself has been differentiated by a process of evolution and so in all probability have the infectious disease germs.

We are thus compelled to dissent from Crookshank's conclusion that cow-pox and small-pox are radically dissimilar diseases. They *may* be radically dissimilar but it appears to me that he has failed to establish that fact on any sufficient grounds. And hence his main conclusion that cow-pox does not protect against small-pox does not follow.

While from a scientific standpoint it would be highly interesting to know the nature of cow-pox and just how and why it operates to protect against small-pox, our lack of such knowledge is a very flimsy excuse for abandoning the operation, if it can be shown by experience that it actually does exert a prophylactic influence over small-pox. We do not know the *modus medendi* of quinine in ague or salicylic acid in rheumatism, but does any one refuse to prescribe these drugs under these conditions on that account?

The time has long since passed when a question of this kind can be settled on theoretical grounds. The operation has been in vogue throughout the civilized world for nearly a hundred years, and we are entirely independent of theory in discussing its efficacy. Vaccination, therefore, must stand or fall according as experience shows that small-pox is less likely to occur in the vaccinated than in the unvaccinated; that small-pox is less fatal in the vaccinated than in the unvaccinated; and that the dangers of the operation are practically nil.

ITS PROPHYLACTIC POWER.

When Dr. Jenner published his account of Variolæ Vaccinæ in 1798 he believed that successful vaccination would confer permanent immunity from small-pox. He said: "But what renders the cow-pox virus so extremely singular is that the person who has been thus affected is forever after secure from the infection of small-pox; neither exposure to the variolous effluvia nor the insertion of the matter under the skin producing the distemper." The early vaccinators were all impressed with this belief. Jenner particularly advised that small-pox inoculation should be performed after successful vaccination in order to put its virtues to the crucial test, and this advice was followed in a great number of cases and with uniformly satisfactory results. But it was not many years before cases of small-pox began to appear among the vaccinated. An investigation of these cases led to a doubt as to whether the victims had really been *successfully* vaccinated, and Jenner was led to believe that they had suffered from what he was pleased to term spurious cow-pox as distinguished from the genuine cow-pox.

The sources of such spurious disease being, as stated by him, "matter arising from pustules on the nipples or udder of the cow, which pustules contain no specific virus; matter (although originally possessing the specific virus) which has suffered a decomposition either from putrefaction or from any other cause less obvious to the senses; matter from an ulcer in an advanced stage, which ulcer arose from a true cow pock." In addition to these sources he referred to certain morbid matters generated by a horse, but which at the present time is of little interest to us. It is perfectly manifest that if a person is vaccinated with matter from any of these sources, a *successful* vaccination can not result. It is well known that vaccine virus is not easily preserved; that a temperature much above 65° F., and moisture rapidly destroy its specific qualities, and that bovine virus loses its properties more readily than humanized. Nevertheless, I apprehend that virus thus rendered specifically inert, and also under some circum-

stances, as when there is a great demand on the propagators for virus, when taken at too late a stage of the disease, is used quite extensively, particularly at times of small-pox scares. The result of vaccinating with this spurious virus is an atypical vesicle, such as most any other local irritant might produce, and an atypical scar, and it does not afford any protection against small-pox. Again, the operation may be followed by a deep, ill-looking, sloughing ulcer, generally as a result of an injury to the sore by which the vesicle is opened and offending matter introduced into the wound from without. In these cases the vaccination should not be regarded as successful, and the operation should be repeated within a short time after the recovery of the patient. Complications of this kind are most apt to occur in people who are careless as regards personal cleanliness.

Ultimately, however, small-pox occurred after perfect vaccination, a fact which some of the early operators were extremely loath to admit, but small-pox itself has not infrequently occurred a second, third, and even fourth time in the same person. And not only is this true, but "many cases of second small-pox have been in those persons who in the first instance had undergone it in the most severe and dangerous form." (Good's Study of Medicine, vol. I., page 634; sixth American edition, 1835.) A notable example of this fact was furnished in the case of Louis XV. of France, who, having previously had small-pox in a severe and dangerous form, died of it in 1774.

When small-pox was epidemic in Marseilles in 1825, 2,000 of the inhabitants of that city had previously had the disease. Of these 20 again contracted it, and four of them died. In 1872, in the Boston Small-Pox Hospital (Boston *Medical and Surgical Journal*, 1872, page 312) there were treated two cases of second small-pox. The first, a woman, died of the second attack twelve weeks after the first; the other was a child one year old. Dr. Webb (Ibid, page 397) saw twenty-three cases where small-pox occurred twice in the same person, and one case where it occurred for the third time.

Dr. Thomson (Watson's Practice of Physic, page 1008, second American edition, 1845), during an epidemic of small-pox in Scotland, in 1818–19, saw seventy-one cases occurring in persons who had previously had the disease, and of which number three died.

The very first men who recognized the disease in Europe also noted facts similar to the above. Thus, Rhazes, Avicenna, and also Ahron, asserted that the disease occasionally occurred a second or even third time in persons apparently having a strong predisposition to it, and Borelli, of Naples, in the seventeenth century, recorded the case of a woman who recovered from seven distinct attacks of small-pox, and died of the eighth. In the London Small-Pox Hospital, in 1844, an Irishman died of the disease who had been successfully vaccinated in infancy, and who early in life had had small-pox.

Inasmuch, therefore, as small-pox occurs, with a certain degree of frequency, for the second time in the same individual, it ought not to surprise any one if it should occasionally occur after a perfectly successful vaccination. It is not now claimed "that the person who has been thus affected is forever after secure from the infection of small-pox," but it is admitted that it *may* thus occur.

It is by no means an uncommon thing for a discoverer or inventor to expect greater things as a result of his invention or discovery than subsequent events appear to justify. And it has been so with the operation under consideration; "we have learned to correct our sanguine views, and prescribe a narrower boundary for the things we expect to attain."

It is astonishing to one looking over the literature of the subject of vaccination to find what an amount has been written upon it, both for and against. And what has struck me as specially remarkable is the incoherance and irrelevancy of a great deal of it. Page after page appear of words—empty words—which, if they convey any idea at all, have no reference to the question at issue. Any ideas that the Chinese may have had a thousand years ago as to the nature of small-pox can certainly have nothing to do with the efficacy of vaccination as a prophylactic against the disease, and yet that subject is brought up in a recent attack upon vaccination. It has been claimed that small-pox was on the decline at the time of the introduction of vaccination; that being a disease of Oriental origin it would naturally follow the history of bubo plague, a disease of the same geographical origin, and retreat from Europe into Central Asia. It is said that in the East tradition pointed to the camel as the original source of small-pox; but the camel inhabited North America ages before he appeared in Asia. It must be remembered that Central Asia was the cradle of the human race; that from ancient Bactriana sprung those races which have since spread over and civilized Europe and America, so that at the present time no civilized country is occupied by the proper Autocthons of the soil. In like manner a number of infectious and contagious diseases other than small-pox may be traced to this geographical origin, notably diphtheria, pertussis, measles, scarlatina, and probably also typhoid fever. These diseases, including small-pox, have shown as yet no more disposition to cease their migrations and return to the locality from which they sprung than has the human race itself, and there is no reason to expect that they will.

It is entirely irrelevant to inquire the number of deaths from small-pox per million of population in any period of years subsequent to the practice of vaccination, compared with an equal period prior to its practice, when, of course, records were everywhere incomplete and statistics thoroughly unreliable.

Nor is it any more pertinent to the issue to compare the deaths per million of population living with the proportion of the vaccinated to the unvaccinated. As a sample of this method of argument I append the following, which I have recently seen in print:

DEATHS FROM SMALL-POX IN SWEDEN.

	Vaccinated per 1,000 inhabitants.	Deaths from small-pox per million inhabt'nts.
1825	250	400
1839	590	575
1851	735	700
1874	970	960

Showing, of course, according to the anti-vaccinationists way of looking at it, that the larger the proportion of the vaccinated in any community the greater the mortality from small-pox.

I have no hesitation in affirming that these figures prove absolutely nothing pertinent to the issue. In 1874 the population of Sweden, in round numbers, was 4,200,000, and hence 150,000 were unvaccinated, and these statistics give us no reason to doubt but

that the entire mortality from small-pox in this year was furnished by the unvaccinated. Hence the uselessness of such tables.

The claim that the mortality from other diseases has increased since the introduction of vaccination, is too flimsy and ridiculous to require a moment's notice. As already stated, vaccination must be abandoned or not according as it shall appear that small-pox is less liable to occur in the vaccinated than in the unvaccinated; that small-pox is less fatal in the vaccinated than in the unvaccinated; and that the dangers of the operation are practically *nil*.

Dr. Woodville, within a short time after the outbreak of cow pox in the London dairies in 1799, was able to report on nearly 500 successful vaccinations. In a large number of these cases, I think 160, the patients were afterwards unsuccessfully inoculated with small-pox virus. Crookshank thinks that most of these cases were in reality inoculated with small-pox virus at the time of their vaccination, inasmuch as more or less extensive secondary eruptions occurred, and that this accounts for their subsequent immunity from the disease. This may be so or may not, and in any event we can get along very well with our argument if we exclude these cases from records as being uncertain. Dr. Joseph Marshall, who in the year 1800 A.D., made an extensive tour, teaching and practicing vaccination, was soon able to report on 423 cases of which he kept careful notes. Of these he afterwards inoculated 211 with small-pox virus, failing to produce the disease in any instance. I also call to your memory the experiment on the six negro children who were shipped on board the vessel "Jeune Caroline," in 1804, to the Seychelles Islands; the inoculation of a number of natives of Burmah last year, after successful vaccination; and the numerous experiments of the same kind, and with the same result, performed by Jenner hinself and his immediate successors, which experiments are at the present time impracticable.

In 1853 the Epidemiological Society of London received two thousand letters from medical men, affirming their belief, from personal observation, in the protective power of vaccination.

Dr. Simon received 540 similar letters from eminent English and foreign physicians out of 542 to whom letters of inquiry were addressed. One of these observers, Marshall, showed that of 757 persons exposed to small-pox 231 had been vaccinated, and of these 27 contracted the disease, whereas of the remaining 526 unvaccinated 519, or all but seven, took it. Dr. W. L. Atlee, of Lancaster, Pa., in 1880, after sixty-two years' experience with vaccination, and having vaccinated and revaccinated thousands of children, affirmed in the most decisive manner his belief in the prophylactic power of the operation, and that there had been no deterioration in the protective influence of the virus during those years. (*Amer. Journal of the Medical Sciences*, 1880, p. 189.) He says: "I have tested the efficacy (of the virus) by inoculating for small-pox after vaccination; have taken patients after vaccination to cases of malignant small-pox in small and hot stove rooms, and exposed them to the foul atmosphere for fifteen or twenty minutes, secure from danger. In one case of a mother with six unvaccinated children, one at the breast, who had a severe attack of small-pox, as soon as I discovered the nature of the case, I vaccinated all the children with a vaccine crust, and they all took the vaccine disease. The room—it was in February—was a small 10×12 feet room, with a hot tin-plate stove and but one bed, on which they all slept, and which was saturated with small-pox contagion; yet these children picked off the scabs from their mother's body, and the baby nursed at her breast, and no one took the disease."

In November last, small-pox appeared among the negroes in the little village of Harris' Neck, Georgia. Passed Asst. Surgeon J. H. White, in command of the South Atlantic Station, U. S. Marine-Hospital Service, was despatched to the locality to investigate the outbreak, and take the steps necessary for its suppression. He reported, November 27, 1891 (Abstract of Sanitary Reports), that fourteen houses were infected. Fifty-six of the inmates of these houses were unvaccinated, and all but one contracted the disease. The one that escaped was an infant 2 weeks old, an age at which children are, as a rule, exempt from all contagious diseases. There were thirty-three inmates of these houses that had been vaccinated, mostly years ago; of these twenty contracted the disease, all having it in a mild form with one exception, that of a woman who had been vaccinated many years ago, and who had corymboid variola at this time.

When small-pox visited Sheffield, Eng., in 1881, there were in that city, in round numbers (*British Medical Journal*, Vol. i, 1888, p. 308), 100,000 children under 10 years of age; of these 95,000 had been vaccinated, and 5,000 were unvaccinated. The 5,000 unvaccinated furnished 172 cases of the disease, whereas the 95,000 vaccinated only furnished 189 cases. During the small-pox epidemic in Marseilles in 1825, already referred to, 8,000 unvaccinated persons furnished 4,000 cases, whereas 30,000 vaccinated only furnished 2,000 cases. These facts indicate unequivocally that the vaccinated are much less likely to contract the disease than the unvaccinated. This was the experience of Dr. Parke, who accompanied Stanley on his recent African expedition. He vaccinated forty native bearers (*British Medical Journal*, Vol. i, 1890, p 1084). On the return, small-pox broke out in the camp, skipping thirty-eight of those vaccinated, and attacked two who had poor scars, but who recovered promptly, while the unvaccinated were dying by scores. The mortality tables of small-pox hospitals show the relative death-rate among the two classes of patients, which can be seen in the subjoined tables. Reviewing them, we observe that the lowest mortality among the unvaccinated was 21 per cent., and the highest mortality among the vaccinated was 19.5 per cent., which, without going further, would show a decided advantage on the side of vaccination. But a further study of the tables shows an *average* mortality among the unvaccinated of 44.5 per cent., whereas among the vaccinated it was but 6.72 per cent., a difference of 37.78 per cent. in favor of the vaccinated.

These tables also show a decided advantage of revaccination, the death-rate among the revaccinated in some instances being less than 1 per cent. We must, therefore, conclude that small-pox is less fatal in the vaccinated than in the unvaccinated.

If the dangers of the operation are so great as to render it a questionable procedure, we ought to be able to derive valuable information on this point from men who have been so situated as to see a great many cases. In 1885, Dr. G. G. Craig, of Rock Island, Ill. (*Sanitarian*, 1891, p. 561), addressed a circular to

the health officers of the cities of the United States containing more than 20,000 inhabitants, asking information on this point. He received replies from eighty-five. Dr. DeWolf, then and for some years previously, Health Commissioner of the City of Chicago, stated that since 1880, 250,000 vaccinations had been done by officers of his department, and that no evil result beyond an ulceration at the seat of the sore following some local injury, or an occasional abscess in the armpit had occurred in the whole number.

Dr. J. H. Rauch, for many years secretary of the State Board of Health of Illinois, was recently examined on this subject by the Royal Commission on Vaccination, he having had in all probability a larger experience in this field than any man in this country. He testified that since 1861 he had personally vaccinated and had under his immediate supervision about 250,000 people; that he had never during that time seen a case of transmitted syphilis; that there had not been a death as the result of the operation; but that he had occasionally seen ulcers at the seat of the vaccination. Here, then, are 500,000 cases of vaccination without a death or any accident worse than an axillary abscess or a slow healing ulcer. The records of the public vaccinators in English cities present about the same facts.

The records of the Royal Health Office of Germany (Reference Handbook of Med. Science, Vol. vii, p. 557), show that more than 2,250,000 vaccinations were performed in the empire in one year. Among these there were four cases of inflammation of the lymphatic glands, all of which promptly recovered. There were no cases of either transmitted or evoked syphilis, but there were a number of cases of erysipelas with eleven deaths. These cases upon investigation were ascertained to be due either to the carelessness of the patients or their parents. Erysipelas is a disease caused by a specific microörganism, and can only result from the presence of this microbe at the seat of the disease; it may follow any abrasion of the skin, not as a *result* of the abrasion but as a coincident event, and this is the case when it follows the operation of vaccination.

Last winter a large number of children were vaccinated in the schools of this city by officers of the Health Department; in one case a rather severe erysipelas followed the operation. Upon investigating this case, it was learned that the mother was suffering from erysipelas at the time the child was vaccinated; that she was anointing the affected parts with an ointment prescribed by her attending physician; that she conceived the idea that this ointment would be a good thing to apply to the child's sore arm; that this idea she carried into effect thereby inoculating the arm with the germs of the disease which promptly made its appearance and was, of course, attributed to the vaccination. It thus appears that among the 500,000 vaccinations reported by Drs. DeWolf and Rauch there were no fatalities; that out of the 2,250,000 German vaccinations there were eleven deaths from erysipelas following the operation, being a mortality of one in 204,500. These figures would not appear to indicate that this operation is one of very great gravity, but they are just about what most physicians would expect judging from his own experience and that of his immediate professional neighbors.

During the first year of life the child is specially subject to various forms of skin eruptions, and if they appear after it has been vaccinated the fact is seized upon as a *propter hoc*, whereas by no possibility can the relationship of cause and effect be established. It has never been shown that any of these skin eruptions are transmitted from one individual to another through the medium of vaccine virus, but in a child constitutionally predisposed to these disorders it is conceivable that the local irritation of the vaccination may call them into existence. This is well recognized by the profession, and hence a physician would be justified in postponing the operation in a child in whom he had reason to suspect that this predisposition existed. Thus in Ireland in the second quarter of 1890, the operation was postponed for various reasons in 2,909 cases.

If, therefore, the operation is not done on young, feeble and sickly children, or during the period of dentition, bad results or accidents will be exceedingly rare. If, on the other hand, such infants are vaccinated it need not excite surprise if, now and then, an hereditary predisposition to disease manifests itself, just as it would after any other affection of equal severity.

There is one disease, however, which is capable of being transmitted from individual to individual through the medium of vaccine virus, and that is syphilis. With us in America this is a question of little importance, as we use almost exclusively bovine virus in our operations. Inasmuch, then, as the cow is insusceptible to syphilis it is impossible to transmit the disease by means of this virus. The facts here brought forward are, I think, sufficient upon which to base the three propositions sought to be established, namely, that small-pox is less likely to occur in the vaccinated than in the unvaccinated; that it is less fatal in the vaccinated than in the unvaccinated; and that the dangers of the operation are practically *nil*.

TABLE I.

In Prussia in 1859 there were (N. A. Med. Chirurg. Review, Vol. V, 1861, page 1101, from report published by authority of Prussian government):

	Cases of Small-pox.	Among the Vaccinated.	Among the Unvaccinated.
	16,025	13,364	2,671
Deaths	1,371	801	570
Per Cent.	8½	6	21

TABLE II.

(N. A. Medico-Chi. Rev., Vol. IV, 1860, page 516. Alex. Wood, President Royal Coll. Phys., Edinburgh.)

Mortality from small-pox in the unvaccinated, taken generally	35%
" " " " children under 5 yrs.	50%
" " " " badly vaccinated	15%
" " " " vaccinated generally	7%
" " " " well vaccinated	1%

TABLE III.

(Lancet, 1863, page 688. Marston, surgeon of Small-pox and Vaccination Hospital, Highgate.)

Mortality of small-pox among the unvaccinated	36%
" " " vaccinated	6.76%
" " " " having 1 scar	7.57%
" " " " " 2 scars	4.13%
" " " " " 3 "	1.85%
" " " " " 4 "	0.74%

TABLE IV.

Small-pox at Latrobe, Penn., 1871 (Transact. Am. Med. Soc., 1872, page 454).

	Total Cases Small-pox.	Cases Among the Unvaccinated.	Cases Among the Vaccinated.
	51	21	30
Deaths	11	10	1
Per Cent.	21½	47½	3½

TABLE V.

(Med. and Surg. Reporter, Vol. XXX, 1874, page 281. Dr. Martin Luther, who was a surgeon in the Prussian army during Franco-Prussian war.) In three German hospitals there were admitted:

	With Small-pox.	Number Vaccinated.	Number Revaccinated.	Number Unvaccinated.
	536	401	110	25
Deaths	34	15	2	17
Per Cent.	6.34	3.74	1.82	68.

TABLE VI.

Philadelphia Municipal Hospital, 1871 (Transact. Am. Med. Ass'n, 1872, page 442).

	Total Cases Small-pox.	Unvaccinated.	Vaccinated in Infancy. Good Mark.	Vaccinated in Infancy. Fair Mark.	Vaccinated in Infancy. Poor Mark.
	1,189	390	331	166	302
Deaths	382	254	33	27	68
Per Cent	32.	65.1	9.9	16.2	22.5

No statistics as to revaccination.

TABLE VII.

Dr. Robt. Grieve, Hampstead, in paper read before the Epidemiological Society of London (Med. Record, Vol. VIII, 1873).

	Total Cases Small-pox Observed.	Unvaccinated.	Vaccinated.
	6,221	1,248	4,973
Deaths	1,205	638	567
Per Cent.	19.38	51.12	11.40

The number of vaccination marks were noted in 3,555 instances.

	Those having 1 mark	2 marks	3 marks	4 marks	5 or more marks
Mortality per cent.,	17.39	12.17	10.58	8.38	6.43

TABLE VIII.

(Lancet, 1871, pages 315-316. Dr. R. Grieve, medical director Hampstead Small-pox Hospital.)

	Cases Small-pox Admitted Dec. 1, 1870—Feb. 18, 1871.	Number Vaccinated.	Number Unvaccinated.
	800	591	209
Deaths	154	58	96
Per Cent.	19.25	9.8	45.8

TABLE IX.

Gladbach, Rhine Province, Prussia. Population, 47,800. (British Med. Journal, Vol. I, 1890, page 872.)

	Total Cases Small-pox.	Unvaccinated.	Vaccinated.	Revaccinated.
	94	8	48	38
Deaths	14	4	9	1
Per Cent	14.9	50.	18.75	2.63

TABLE X.

Drs. Seaton and Buchanan (Reynolds' System of Medicine, Hartshorne's, Vol. I, page 369), during an epidemic of small-pox in London in 1863, examined over 50,000 children in various institutions.

360 per 1,000 having no vaccination mark had small pox.
6.8 " " 1 " " " "
2.49 " " 2 " marks " "
1.42 " " 3 " " " "
0.67 " " 4 or more vaccination marks had small pox.

TABLE XI.

(Boston Med. and Surg. Jour., 1872, page 308.) Records of Boston Small-pox Hospital.

8 per cent. of cases of small-pox died who bore any mark of vaccination.
67 " " " " " " no " "

TABLE XII.

(British Med. Jour., Vol. I, 1888, page 151. Dr. Tomkins, Montreal Epidemic of 1885-86.)

	Total Small-pox Cases in Hospitals.	Unvaccinated.	Vaccinated.
	1,332	805	527
Deaths	418	315	103
Per Cent	31.3	39.1	19.5

Under 10 yrs. of age . 489 } Mostly unvaccinated.
Deaths . . . . . 202 }
Per Cent . . . . . 41.3 }

Over 10 yrs. of age . 843 } Mostly been vaccinated once.
Deaths . . . . 216 }
Per Cent. . . . 25.62 }

TABLE XIII.

(Brit. Med. Jour., Vol. I, 1888, page 308, editorial.) Sheffield, Eng., Recent Epidemic of Small-pox.

100,000 children under 10 years of age.
95,000 were vaccinated. Furnished 189 cases small-pox, 2 deaths.
5,000 were unvaccinated. Furnished 172 cases small-pox, 70 deaths.

London, 1881.
916,000 children under 10 years of age.
861,000 were vaccinated. Furnished 125 deaths from small-pox.
55,000 were unvaccinated. Furnished 782 deaths from small-pox.

TABLE XIV.

(Sanitarian, April, 1891, page 365. Dr. Barry's Report of the Small-pox Epidemic in Sheffield, Eng., 1887-1888.)

101 per 1,000 unvaccinated children under 10 years of age had small-pox, and 44 per cent. died.
5 per 1,000 vaccinated children under 10 years of age had small-pox, and 9 per cent. died.

TABLE XV.

Small-pox and Vaccination Hospital, London. (Reynolds' System of Med., Vol. I, Hartshorne's.)

	Total No. Unvaccinated from 1836-1851.	Total No. Vaccinated from 1836-1855.
	2,654	4,896
Deaths	996	316
Per Cent	37.	6.56

TABLE XVI.—(Ibid.)

	Having 1 Good Vacc. Mark.	Having 2 Good Vacc. Marks.	Having 3 Good Vacc. Marks.	Having 4 or More Good Vac. Marks.
	1,032	873	307	358
Deaths	39	20	3	2
Per Cent	3.83	2.32	0.99	0.55

TABLE XVII.

Small-pox in Marseilles in 1825. Population, 40,000. (Watson's Practice of Physic, page 1,008; 2d Am. ed., 1845.)

	Vaccinated. 30,000	Unvaccinated. 8,000	Previously had Small-pox. 2,000
Contracted the Disease	2,000	4,000	20
Deaths	20	1,000	4
Percentage of Deaths among those Attacked	1	25	20

TABLE XVIII.

Small-pox in Scotland, 1818-1819. Dr. Thomson saw in this epidemic 586 cases. (Ibid, page 1008.)

	Vaccinated. 310	Unvaccinated. 205	Previously had Small-pox. 71
Deaths	1	50	3
Percentage of Mortality among those Attacked	.32	24.39	4.22

TABLE XIX.

Small-pox in Hamburg, August 19 to November 18, 1871. (N. Y. Med. Journal, 1872, page 213.)

	Vaccinated. 2,954	Unvaccinated. 1,010
Deaths	347	700
Mortality	11.74	69.30

TABLE XX.

Small-pox in the Philadelphia Municipal Hospital, 1872. (N. Y. Med. Record, 1872, page 166.)

	Vaccinated. 550	Unvaccinated. 677
Deaths	8	353
Mortality	1.45	52.14

TABLE XXI.

Small-pox at Harris' Neck, Georgia, 1891; 14 Infected Houses. (Abstract of Sanitary Reports, Nov. 27, 1891. Published by U. S. Marine Hospital Bureau.)

	Variolated. 12	Vaccinated. 33	Unvaccinated. 56
Cases of small-pox	None	20	55
Deaths from small-pox	"	Not stated	13
Mortality per cent.		"	†23.63

For the past twenty years bovine virus has been used very largely in the United States, and at the present time nearly all physicians use it to the exclusion of humanized virus. The propagation of animal virus, though in vogue for many years in Italy, was not practiced until the year 1866 elsewhere. Troja, of Naples, inoculated a heifer with virus from the arm of a child, and thus by retro-vaccination initiated a stock of vaccine virus which he propagated from animal to animal for a number of years. His neighbor, Negri, continued the work until 1820, when he encountered a case of natural cow-pox in Calabria and after that time propagated virus from that source.

An unsuccessful attempt was made in 1864 to introduce bovine virus into France. In 1866, in the month of March, a case of natural cow-pox appeared in the town of Beaugency, France, about fifteen miles from Orleans. The disease being recognized, Depaul, the director of the government vaccine establishment in Paris, was notified, and he visited the case, securing some of the virus, with which he began vaccinating animals in succession. Virus from this source was soon in use throughout France. In 1870, Dr. Henry A. Martin, of Massachusetts, received a supply of this Beaugency virus, which he has since propagated, and which for a number of years was the only bovine virus used in this country.

Dr. Griffin, of Fond du Lac, Wis., established stables for the propagation of animal virus in 1872, using Beaugency stock. Dr. Griffin later, 1880, imported virus from Brussels. This is the Belgian stock, for a number of years propagated by Dr. Warlomont, director of the vaccine establishment at Brussels. This stock was derived from a case of natural cow-pox occurring in the neighborhood of Esneaux in 1868.

Dr. Griffiths, of Brooklyn, N. Y., began in 1878 propagating virus derived from a case of natural cow-pox occurring somewhere in the interior of the

State. The various vaccine farms in this country at the present time are propagating virus from one of these three stocks (DeWolf, *Chicago Med. Journal and Examiner*, 1881, vol. xlii, p. 481, ex seq.), all of which have been shown to be vigorously prophylactic against small-pox.

In seasons when small-pox is prevalent, there is always a great and unusual demand on the propagators for virus, and the temptation is necessarily great to make every animal produce the largest possible amount of lymph. It may thus happen that points are charged with a material from a vesicle at too late a period of the disease, or the discharge may be increased by irritating the parts so that points charged under such circumstances, will not produce the genuine and protecting vaccine disease, but a spurious cow-pox affording no such protection.

It would, therefore, seem that it would be eminently advisable and proper that vaccine farms should be under government supervision, as it is certainly an injustice to vaccination to be held responsible for the shortcomings of propagators. Dr. Craig, of Rock Island, Illinois, ten years ago strongly urged this point before the Illinois State Board of Health.

803 Grand Ave.

SOME CONTRA-INDICATIONS FOR THE USE OF OPIATES.

Read before the Philadelphia County Medical Society, January 27, 1892.

BY MARIE B. WERNER, M.D.

My object in presenting this subject before the Society to-night is manifold; I wish to show that in general practice the indications are limited for the use of opiates. We must fully realize that we have a broader basis for medical science than symptomatology, in order to give our patient the full benefit of our knowledge. Our position being at all times one of trust, we must endeavor, in helping our patient, to find the cause and remove it, rather than hide the symptoms it gives rise to by an opiate. Aside from the possible mental disturbances such a course of treatment might induce, it often materially complicates surgical efforts to relieve patients, the results of which we have repeatedly heard discussed at these meetings.

In order to discuss this drug with due fairness, it will be necessary to give a few moments to the consideration of its physiological action on the human economy. Dr. H. C. Wood, in his *Treatise on Therapeutics*, says: "When opium is taken in such a dose as to produce its mildest physiological effects, it exerts a quieting influence, inducing a peculiar dreamy condition; after a length of time varying according to the idiosyncrasies of the patient and the dose of the drug, this condition passes into sleep, either light, dreamful, natural, or heavy and deepening into stupor. On awakening, the patient may return at once to his normal condition; but very often he experiences a state of depression shown by languor, a little headache, nausea, or even vomiting, which may last for some hours. One of the most common of these departures from the ordinary course of symptoms is an excessive depression following the sleep produced by moderate doses of the medicine. This state is seen, as far as my experience goes, most usually in females of weak, nervous organization, such as are peculiarly liable to attacks of neuralgia. The symptoms are a feeling of weakness and prostration, often accompanied by chilliness, dull headache, and giddiness, but especially marked by intense nausea and frequent vomiting."

Bartholow states that, as a rule, opium does harm in all gastro-intestinal maladies in which there is a deficiency in the proper secretion, or a suspension of the functions of the liver and kidneys.

Dr. J. B. Mattison, in his valuable paper read before this Society in October, 1890, entitled "The Renal Status of Opium Habitués," after a careful analysis, arrived at the following conclusions: *First*, the habitual use of opium in any form will cause organic renal disease; *second*, the changes most likely to be met with are cirrhotic; *third*, that the rationale is threefold. Vasomotor changes, impaired general nutrition and inflammatory action due to non-eliminated irritant products. I would further call attention to the valuable contributions of Dr. A. Haig, published in the *British Medical Journal*, 1890, in his studies on the influence of opium and morphine on uric acid; also, of the retention of this latter product in the human economy and its relation to the causes of disease. His observations and experiments proved to him that the administration of opium or morphine caused retention of uric acid, accompanied by a reduction of arterial tension; that, when the effect passes off, there is a rebound, with an excessive excretion of uric acid and marked high tension, often accompanied by headache and mental depression.

Let us look carefully into these statements and compare them with our practical experience. We find that, after lulling pain and induction of sleep, we come to a period of depression, even after a moderate dose of the drug. This depression is usually followed by a certain loss of resistance to bear any renewal of pain, and, in consequence, it becomes necessary to repeat the use of the drug; indeed, when we study carefully its action on a previously weak, nervous organization, we find the description given by Dr. Wood very ably described the case for us. He tells us: "The symptoms are a feeling of weakness, prostration, chilliness, dull headache, nausea," etc. In the face of this the questions must certainly present themselves to us: Is it wise to simply gratify the desire of the patient? Would it not be better to study the cause and remove it rather than hide the symptoms which lead us to the origin of the trouble? I refer to cases in every-day practice; cases in which a periodical monthly pain is lulled to sleep by several doses of morphine or opium, while the proper cause is entirely left out of sight. There is no doubt in my mind that cases have come to all of us where a case of chronic constipation or continuous indigestion combined with a nervous, irritable temperament, perhaps, added to that, or independent of this, an unpleasant skin eruption, etc., claimed our attention.

Quite often a question regarding any menstrual difficulties shows that there is some pain, often varying in severity, and the next question, What do you do for the pain? will elicit the answer: Oh! I take a little paregoric, or I have some pills or suppositories I use, perhaps previously given to some member of the family for pain. Not infrequently an investigation will show that the main ingredient is some opiate.

In our day of progressive medical science it becomes necessary that we should join hands and forces —first, to see if the proper hygienic rules regarding

clothing, exercise, and cleanliness are scrupulously carried out, and that the proper functions of secretions and excretions are thoroughly understood. If aside from these precautions there is still pain, a local investigation should be made by the physician, and, the condition carefully studied, the cause removed if possible. To give an opiate in such cases I consider criminal, since the patient receives a double injury; not only is she not relieved permanently, but she is robbed of much of her normal resistance, and I fear many have become chronic invalids, for it is a constant struggle to overcome the after-effects of the opiate before the time arrives for another relief of the same sort. If the patient escapes becoming addicted to the opium habit, she cannot escape the local gastro-intestinal irritation which is invariably set up, and which will defy all medication so long as its cause is kept up.

I can at this moment call to mind three cases in which three weeks out of four all sorts of laxatives are used to overcome the amount of morphine taken during the fourth week; the complexion is sallow, the breath heavy, the skin impure, and how can it be otherwise? If an examination reveals no functional trouble other than the local congestion, or possibly some displacement downward induced by improper clothing or a lack of attention to the proper excretions, is it not at once clear that an opiate in the long run increases the cause of pain? Lulling the pain induces no cure, and the resulting constipation acts in two ways to make the patient worse: first, the pressure of a distended bowel; second, the absorption of effete products. Let such conditions continue for some years, as they often do, and we have other factors enter the field to make life miserable—sluggishly acting liver or kidneys, a worn-out stomach, and not infrequently a nervous wreck.

In order to emphasize this point, I will only have to call attention to a series of comparative experiments on animals[1] made by Dr. Edward Levinstein, and reported in his book on "Morbid Craving for Morphine." His deductions from a number of experiments are as follows:

"1. That internal applications of morphine sooner paralyze the digestive powers of the stomach than the subcutaneous injection.

"2. Both ways of administering morphine bring on functional disorders of the secreting nerves.

"3. Both cause catarrh of the stomach and intestinal tract.

"4. Large doses of morphine given internally cause a subacute catarrh of the stomach, on account of the irritating chemical action of the morphine.

"5. The subcutaneous injection of morphine causes a chronic catarrh of the intestines in a mechanical manner; in consequence of the impaired influence of the secreting glands due to the action of the morphine, the secretion of the digestive fluids is stopped altogether, or at least diminished in quantity, and consequently the intestinal tract is encumbered for a longer time by the ingesta."

The same author speaks of amenorrhœa and sterility as being a sequence to the continued use of morphine, drawing largely upon the results of his own observations, and accepts Pflueger's theory in explanation of it.

In these days, when "preventive medicine" is being advocated by all of us who desire to place medical science on the highest standard, should we not think many times more than twice before we write a prescription for an opiate to relieve pain? In the face of all these facts, it becomes a serious question of right and wrong if we stop short of exerting all our knowledge to study the *cause* of the pain we have been called in to alleviate. Often the prescription-book is entirely useless, unless its blanks could be filled with directions to the patient how to dress, eat, and give herself the physical care she needs.

The use of morphine after pelvic operations has been discarded by most of our operators, and clearly has been the means of reducing mortality rates, as well as obviating many of the dreaded after-complications. I recall in one of my earlier operations the advice given by one of our older physicians to rely on opium and calomel, which I followed, with the result of having on my hands a sufferer from insomnia and chronic constipation after I had discontinued its use; had I not been careful to destroy all prescriptions, I feel certain I would have had more trouble. Another case comes to my mind of a patient who had a section done for some pelvic trouble by a physician who also believed in the opium after-treatment. This patient came under my care later, and confessed that she had often helped herself to a suppository after the doctor had stopped their use. My object in referring to these cases is to show the danger of setting up the morbid craving and its attendant evils with only medicinal doses and in the space of two or three weeks; showing at once the danger a prescription containing an opiate may induce in the hands of a nervous patient who has periodical attacks of pain. Such cases do not always reach the state necessary to require hospital treatment; hence, are often exceedingly vexing to the physician and surrounding friends. A direct accusation to the patient would often fail to bring the results desired, while the friends and relatives cannot always be relied upon for the tact and discretion so necessary. For that reason the physician must often exercise a vast amount of patience and time to educate the ones immediately concerned, and to prove the deleterious effects the use of opiates have, and then, *perhaps*, the patient can be cured.

It may, perhaps, be of interest to quote from a discussion on morphine in the British Gynecological Society, 1889: Dr. Bantock gave as his experience after surgical operation "that patients were much better off without it—they escaped the restlessness which was left as the opium wore off." Dr. Bedford Fenwick called attention to the fact that opium increased the congestion of the kidneys to a dangerous extent, and might even go to a complete suppression of urine; also, that it caused a complete atony or paralysis of the muscular tissue of the intestines, thus preventing their acting. Dr. R. T. Smith had given two doses of a quarter-grain of morphine each in a case of severe shingles; the patient had suppression of urine for twenty-four hours. Dr. Thomas Savage, in his address read at the annual meeting of the Birmingham and Midland Counties Branch of the British Medical Association, says: "It is not long since it was the custom to administer opium and morphine as a routine treatment in all cases of peritonitis and many other conditions in the abdomen. We have now learned the inadvisability of so doing. May we not extend the withholding of these and similar drugs in other states? I have myself thought

[1] Covering a space of time from six days to five weeks.

that the general practitioners rely too much upon anodynes."

In this matter of too sympathetic and assiduous medical treatment errors rather of judgment than intention are often committed.

Of no less importance is the behavior of an opiate on a patient of uric acid diathesis, in which a demand for relief of pain on the part of the patient often becomes quite urgent. Here, again, the researches of Dr. A. Haig show us that the drug tends to store up the acid; that when elimination begins to take place there is often a return of pain, the patient again demanding relief — in this manner a cycle can easily become established. These pictures teach us the importance of keeping the drug *entirely out of the reach of the patient*, and the necessity of its *careful* and *conscientious* use where it may be indicated.

SOME NEW STUDIES OF THE OPIUM DISEASE.

Read before the Philadelphia County Medical Society, January 27, 1892.

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As a preface, I wish to express my emphatic dissent against the common use of the word *habit*, in describing the opium disease. The popular meaning conveyed by this term is some state or condition voluntarily acquired and retained, with the certainty of being thrown off at any time at the will of the patient. This view assumes a knowledge of the physiology and psychology of the brain and its functions that is not yet attained. Hence the use of the word is incorrect, wrong, and contradicted by the facts in the clinical history of each case. It also conveys a false impression of the nature and origin of such cases, and is a word to which different meanings will always be given. No other word is more misleading and confusing, when applied to opium, alcohol, and other border-land neuroses.

Beyond all question, the toxic use of opium and its alkaloids is rapidly increasing. Only about 50 per cent. of opium and morphine manufactured is required by the legitimate demands of medicine and pharmacy. The enormous balance is consumed in some unknown way. Comparative estimates make the number of opium cases in this country over a hundred thousand. Whether this is correct or not, it is evident that the number is very great and largely concealed, and many of them are very hopeless and difficult to treat. The natural history of such cases indicates a steady, progressive degeneration, on to death. Recovery is rarely spontaneous, and without the aid of applied science. Up to the present time, all clinical studies have been confined to the symptoms and treatment, starting from some indefinite point after the opium addiction begins. The old superstition of a moral origin, and of some wilful, wicked impulse, is accepted as the first original cause. Writers, and even specialists, seldom go back into the early etiology, or inquire what conditions or forces led to the first use of opium. The object of this paper is to trace some recent facts which throw new light on this unknown stage of etiology.

From a careful clinical study and grouping of the history of a number of opium cases, it is evident that a large proportion have a distinct *neurotic diathesis*, or, more literally, have inherited from their parents some condition of brain and nerve defect which favors and predisposes to the development of neurotic diseases. A more careful study of these records shows that in some cases an *opium diathesis* is present, or a special inherited tendency to use opium. Here are two conditions which influence and favor this disease. It is a well-known fact, that a large proportion of all nerve and brain diseases appears in children of neurotic and defective parents. Such children have received some special tendency and predisposition favoring the growth of nerve diseases, springing into activity from the slightest causes.

The latency or activity of this diathesis will depend on certain conditions of life and surroundings, which in many cases can be traced. In some instances the diseases of parents reappear in the children, in others in allied diseases, and not infrequently these defects pass over and reappear in the third generation. Often such defects are dormant, and only break out from the application of some peculiar exciting cause. Thus, a hysteric mother and paranoiac father were followed by three children. One was an alcoholic, the second was a wild, impulsive temperance reformer, the third was a sad, depressed, melancholic man. In the third generation opium and alcoholic inebriety, insanity, pauperism, also feebleness of mind and body, appeared. These varied forms of nerve diseases all had a neurotic diathesis as a basis, and the different phases were the direct result of different exciting causes. These facts are numerous and well attested, and so uniform in their operation, that it is entirely within the realm of possibility to predict, from a knowledge of the diseases of the parent and the environment of the child, that certain forms of degeneration and disease will appear with almost astronomical precision. This term neurotic diathesis covers a vast, unknown field of causes, which extend back many generations. The evolution of brain and nerve defects can often be traced through the realms of environment, nutrition, growth and development. Medical text-books and teachings which fail to recognize this, give very narrow conceptions, and strange exaggerations of the influence and force of many insignificant and secondary factors in the production of disease. The opium-taker has often this neurotic element in his history. It may be traced back to his ancestors, or it may be associated with brain or nerve injuries, cell-starvation, faulty nutrition, auto-intoxications, brain strains, or excessive drains of nerve force. A train of predisposing causes may have been gathering for an indefinite time back. Then comes the match which kindles or fires the train of *gathering forces*. This same train of exciting causes may not explode, because the germ soil is absent. Opium in all forms is given daily, and yet only a comparatively small number of cases become addicted to its use. Why should an increasing number of persons take opium continuously for the transient relief it gives? Why should the effects of this drug become so pleasing as to demand its increased use, irrespective of all consequences? The only explanation is the presence of a neurotic diathesis, either inherited or acquired. The existence of a special opium diathesis has been doubted with supercilious contempt by many writers. Any clinical study will show the error of such doubt. The notes of a few cases which have recently come under observation are illustrations by no means uncommon, and indi-

cate the concealed factors of disease in many instances.

In Case 1 the mother was a secret morphine-taker, the father was a hypochondriac and melancholy clergyman. Two children followed, who were highly educated and healthy. One, a boy, became a physician, and at 30 suddenly began to use morphine, and soon became a chronic case. The other, a girl, was well up to her marriage at 24, when she began to use opium, for no apparent reason.

Case 2, reported by the late Dr. Parrish. Both parents used opium for sleep and neuralgia, and died leaving three children under 5 years of age. They were brought up in temperate families, and had no knowledge of the opium addiction of their parents. One, at 20, continued the use of morphine after it had been given for some intestinal trouble. The second child suffered from dysmenorrhœa and began to take morphine for this trouble, and became a morphine maniac. The third child was a druggist, who at 30 was a confirmed opium-taker.

Case 3. Both parents were neurotic, and probably opium-takers. Both died, leaving an infant child, which was excessively irritable and peevish. By accident, morphine was used as a remedy, and from thenceforth the child would become delirious unless morphine was given daily. All efforts to break up its use failed, and for five years increasing doses were used constantly until the child's death.

In Case 4, five children of unknown parentage were all opium-takers; all lived in different conditions and had different occupations. Two began the use of opium from some bowel trouble. Two have been under treatment, and relapsed (?).

The relief which this drug brings on all occasions, and its impulsive use, are unmistakable indications of a distinct opium diathesis. I believe a careful clinical study will reveal many such instances.

There is a large class of opium cases in which a complex diathesis exists—particularly following inebriety and various forms of brain exhaustion. Often alcoholics will use opium irregularly and transmit to their descendants a diathesis which very commonly favors the use of this drug. Thus the alcohol diathesis frequently becomes the opium craze, with but slight exposure. Both of these disorders are rapidly interchangeable. The children of opium-takers may turn to alcohol for relief, and *vice versa*. It is clear that the moderate use of alcohol produces a degree of degeneration that frequently appears in the next generation as predisposing causes to the opium or allied diseases. Clinical study of cases brings ample confirmation of this. The children of both alcohol and opium inebriates display many forms of brain degeneration. The paranoiacs, criminals, prostitutes, paupers, and the army of defects, all build up a diathesis and favoring soil for the opium craze. Descendants from such parents will always be markedly defective. They are noted by brain and nerve instability, hyperæsthesia, and tendency to exhaustion; also extreme pain from every degree of functional disturbance, with low powers of restoration, inability to bear pain, and suffering from mental changeability, impulsiveness and drug credulity, etc.

These characteristics are prominent, and mark a neurosis that quickly merges into the opium disease. Yet a minority of these cases show a sensitiveness in the effects of opium that prevents them from using it. I have seen a neurotic patient become dangerously narcotized by the use of half a grain of solid opium. Some of the alcoholics and other narcomaniacs have exhibited an incompatibility to opium that is often startling. The emesis and prostration, and the brain-stimulation which approaches and becomes hyperæmia from one or more doses, are familiar to all. This intolerance precludes the use of the drug, and is recognized with alarm by the patient. On the other hand, when the effects are rapid and marked, relieving pain or restoring the disturbance of the functions with no other than a pleasing sense of rest and cure, a dangerous diathesis should be suspected. While the physician recognizes the constitutional incompatibility in one case, he ought not to overlook the abnormal attractiveness of the drug in the other. The dose of morphine which gives the first complete rest, or calms the delirious excitement, or relieves the neuralgic pain or the digestive disturbance, soon calls for its repetition, and many physicians will unconsciously sanction and advise its use. Thus, far more fatal conditions are cultivated and roused into activity. In all neurotic cases, the use of opium in any form when given, should be concealed and watched with care. If a special predilection for this drug appears, equal care and skill should be exercised to divert and change it. Opium should only be used from a knowledge of the nature and character of the case. I have seen the most disastrous results from the reckless use of morphine with the needle. Recently, a man to whom morphine was intolerant was cut and stunned by a falling plank in the street. The surgeon gave him a hypodermic of morphine and ordered him to the hospital. He died in a short time from opium neurosis. Police surgeons often make this mistake, giving morphine that from some unknown reason becomes fatal.

There is another class of opium-takers in which abnormal nutrition seems to be the most active factor in the causation. The neurotic or opium diathesis is not apparently present, and opium-taking dates from some nutrient disturbance. Such cases are very commonly sufferers from dyspepsia, derangement of the liver and bowels. They have a deranged appetite, headaches, cramps, thirst, and fever at times, with nausea. They are anæmic and hyperæsthetic, and complain of varied pains and neuralgias. These cases are evidently ill-nourished, and, in all probability, suffer from imperfect digestion, assimilation, and elimination of food products and waste material. Poisonous compounds and auto-intoxications form sources of serious trouble. The brain suffers from fatigue and pain, the cells are imperfectly nourished, and congestions, complex neuralgias, nerve irritation and instability follow. For this condition opium is almost a specific paralyzant. These cases are found among the over-fed, and those who neglect common hygienic rules of living. Cases of the over-fed are usually epicures, gormands, and persons living sedentary lives, and eating at all times and places. Dyspepsia and derangement of the bowels and kidneys make them drug-takers; then follows opium in some form. Defective elimination and auto-intoxications are always present. The under-fed are usually misers or persons very poor and very neglectful of themselves, or paranoiacs who have some food delusion. They are practically suffering from cell and tissue starvation and nutrient debility. The same dyspepsia and bowel derangements follow. Then follows drug-taking or special foods, and soon opium is dis-

covered and adopted as a remedy. The same poisonous waste products appear from deranged assimilation; also, elimination and the nerve centres are deranged by these new and dangerous chemical compounds. The class of persons who, from simple neglect, become diseased, are often the very poor and ignorant, or some division of the great army of borderliners, who live both mentally and physically on the very frontiers of sanity and insanity. Such persons clearly suffer from many and various forms of autointoxications, and this is proven inductively by the result of eliminative treatment. In all of these cases of nutrient neglect, many favoring conditions encourage the use of opium. These cases are numerous and comprise a large part of the invalids, hypochondriacs, and chronic drug-takers who are seen in our offices and at the dispensaries. They are all practically suffering from faulty assimilations, and faulty eliminations and the irritation of retained poisonous compounds. Opium is a remedy of positive force in covering up the protests of the defective cells and irritable nerves. Often these cases are concealed and are partly the result of previous disorder, and partially acquired from the effects of opium.

Next to this class of nutrient sufferers who become opium-takers, are those who have some entailment of disease or injury. In their history it will appear that some stage of invalidism was present, dating from brain, nerve, or bodily injury. Fevers, heat, or sunstrokes, brain shocks from any source which are followed by unconsciousness, or marked mental perturbations, with exhaustion, and also a profound lowering of all the vital forces. These and other events have left damaged functional and organic activities, manifest in various neuralgias and physical disturbances.

The use of opium conceals and covers up this trouble. Many veterans of the late war have become opium maniacs for the relief of their pains and sufferings, and this is often concealed where it might possibly peril the procuring of a pension. The pension bureau should recognize the use of opium as a natural sequence and entailment following the disease and injury in the service. In Prussia both alcohol and opium inebriety are treated as diseases when occurring in the army or civil service. The suffering and hardships growing out of the war has been the exciting cause of a great many opium cases. Many persons who have no special nerve diathesis in their history, after some severe illness, injury or mental strain exhibit a degree of nerve instability and feebleness that is significant of serious organic change. Such persons manifest perversions of taste, with delusions of foods and medicines, and are on the borderlines of narcomania, ready to use any food or drugs which will bring even transient relief. The use of opium is always perilous. Why all these and similar cases do not become opium takers, is owing to the absence of some diathesis inherited or acquired.

We can see some of the many complex causes favoring brain and nerve strain, with rapid exhaustion and degeneration, and the interchangeability of nerve diseases, in which the use of opium is only another form of the same disease. But we cannot yet trace the early causes and cell-conditions which develop the opium craze. This morbid impulse, like the delirious thirst for water on a desert plain, completely dominates all reason and so-called will-power, and every consideration of life and surroundings. It is more than an accident, more than a failure to reason and act wisely; it is a disease, an organized march of dissolution. The demand for opium is only a symptom; the removal of opium is not the cure. Some central brain degeneration has begun and is going on. Narcomania, a morbid thirst for any solids or fluids that will produce neuroses, is the general name, and opium mania is only one member of this family.

In this study the fact is emphasized that the opium disease appears most frequently in persons who have a neurotic and opium diathesis, also in persons who are suffering from nutrient disturbances, and those who are invalids or have entailment of previous disease and injury; also that certain diseases and symptoms seem to furnish favoring soils for its growth and development. While these are but faint outlines of many unknown facts, they are urged as starting-points from which to base other and more accurate studies. The medical treatment from this point of view is very suggestive. Obviously the removal of the opium is not the cure. The various methods of removal detailed with great exactness, as if they would apply to each case, are unfortunate reflections of the failure of the writers, and are based on the assumption that all cases are the same, and the removal of opium is the great essential in the treatment. Basing the treatment on the clinical study of the case, it will be evident that where an opium diathesis exists, the withdrawal of opium should be very gradual. The treatment and surroundings should be arranged with great care and exactness. Such persons should live in an institution for years or be under constant medical care. The danger of relapse and the future of such cases will depend entirely on the conditions of life and surroundings. Rapid reduction and heroic treatment is never permanent, even with the consent of the patient. Specifics, faith cures, or any measures that promise speedy cure, are failures from the beginning. The road back to approximate health is straight and narrow, and only along lines of applied science. Where the history of a *neurotic diathesis* is present, the withdrawal of the opium should be equally slow.

More attention must be paid to the brain and nerve nutrition. The removal of opium may be followed by the appearance of very serious disorders, such as epilepsy, hysteria, complex neuralgias and paranoiac phases, alcoholism, and various other neuroses. The slow withdrawal of opium enables one to discover and anticipate these neurotic troubles which have been masked before. In one case, suicidal melancholy; in another, in hyperæmia of the brain, with delusions; in the third, irritation and delirium; in the fourth, hysterical spasms appeared when the opium was removed. I have seen two cases of general paralysis suddenly spring into great activity, after the opium was taken away. This condition was not suspected before. Alcoholism is a very common sequel after the removal of the opium. *Cocaine*, *chloral*, and almost every drug that has narcotic properties are also very common entailments. While these are extreme cases, they are likely to be formed at any time. Great care should be exercised in using other narcotics to lessen the irritation from the withdrawal of this drug. Foods and tonics should be given. These cases require the same general treatment as neurasthenia and other states of brain exhaustion. They are drug-takers and will resort to anything for relief. They are secretive, and require more care and more

mental remedies, with long, exact hygienic surroundings.

Where the opium addiction has apparently come from bad nutrition and faulty elimination, with auto-intoxication, the treatment is very hopeful. A long preliminary course of baths, mineral waters, and tonics should precede the removal of opium. Then the opium may be removed at once, without the knowledge of the patient. In proper surroundings with frequent baths, little danger of relapse or suffering will follow. Careful study and treatment of nutrition and digestion will fully restore the case, and relapse seldom occurs except from failure or neglect of the surroundings.

In the last class, where opium is taken and apparently follows from the entailment of some injury or disease, or the exhaustion of old age, a preliminary treatment seems to be required. Often the opium can be abandoned at once for some milder narcotic, and from this, by gradations, discontinued entirely. Full knowledge of the diseased states present will always suggest the lines of treatment. In some cases the opium should not be removed; its diminution and concealment is required. In others its rapid removal is essential. Many varied and difficult questions will appear in these cases. The more accurately the diseased states, also predisposing and exciting causes, the diathesis, and varied influences which have caused opium to be used, are studied, the more accurate the treatment. As in many other diseases, the causes may be anticipated, also neutralized and prevented. Opium-taking should be seen as a symptom; remove or break up the cause, and this symptom disappears.

Routine treatment, either by slow or rapid reduction of the opium, is not wise. The substitution of other narcotics is equally unwise. In a certain number of cases the withdrawal of opium only unmasks more serious diseases, and is positively wrong. A case of general paresis is now under treatment for the opium addiction. Before this opium addiction began the patient caused great distress by his delusions and extravagantly strange conduct. This treatment is wrong. A rheumatic woman of seventy is going through the same course to be free from opium, which has made life tolerable for ten years past. The treatment of opium mania is something more than the application of means and remedies for withdrawal of the drug with the least suffering. The symptomatology and organic lesions often date back to other causes more complex than opium. The treatment must begin by their removal. The general or special diathesis must be treated; the nutritive disorders, intoxications and starvations must be recognized and removed. The influence of pathological states from previous disease and injury must be ascertained and treated. The power of environment, climate, occupation, and idiosyncrasies are also powerful factors to be considered.

These are the essential facts and conditions which must enter into the practical treatment. Among the many important problems, that of prevention promises the greatest possibilities. A recognition of the neurotic diathesis and other predisposing causes would enable the physician to successfully guard its approach.

The successful *stamping* out of both this and the alcoholic disease will be a reality in the future.

It is evident that the opium disease is still an undiscovered country, and the few student experts have not yet passed beyond its frontiers. This disease is all about us and may invade our homes and firesides any time, and *hence* demands recognition and most careful study; above all, ethical and moral levels. Its laws of growth, development, treatment, and curability all follow the great highway of evolution and dissolution.

DISCUSSION.

Dr. Thomas J. Mays: This theme has been one of intense interest to me, but I do not feel prepared to discuss the subject. There is, however, one point to which I should like to refer, and that is the relation between opium-taking and pulmonary consumption. We know that there is a strong affinity between alcoholism and consumption, and I would ask Dr. Crothers whether he has observed a similar relation between opium-taking and pulmonary consumption.

Dr. Joseph Hoffman: Looking at this subject from the medical side, I would say that I have taken some trouble to ascertain the frequency with which opium is prescribed by the general practitioner. The ordinary prescription-file of the drug stores will show that three out of five of the prescriptions contain opium in some form or other, and I think that more than a little of the responsibility for opium-eating lies at the physician's door. It is prescribed without any attempt at cure, for opium has few curative effects, especially in acute disease.

The question arises, When shall opium be used in surgical work? In shock opium will not effect a cure, except in so far as it relieves pain, and if pain is the only thing to be remedied, often we have the remedy in opium. In cases where the pain is self-limited there is a great divergence of opinion as to whether opium should be used or not. In abdominal surgery we must refuse to use it. In these cases opium, instead of being beneficial, is harmful. Although it lulls pain, it produces much after-discomfort. There may be cases in which you cannot do without it; these are the undiscovered cases of opium-eating. In these cases the surgeon has to choose the lesser of two evils. Opium is injurious not only from its physical effect, but also from its moral influence. Take a patient broken-down with pain, add the pain of operation, and give a drug which destroys the power of self-control, and you introduce a semi-hysterical element which causes unending trouble.

In surgery there are better anodynes than opium. I have found the hydro-bromate of hyoscine to be preferable; it does not cause constipation, and thereby increase the tympany, as does opium; its only bad effect is to diminish secretion. By avoiding the use of opium we secure benefit by toning up the patient. In shock, strychnine will at once put the patient to the point of enduring pain without any subsequent discomfort.

Dr. M. Price: The responsibility of the opium habit can, I think, be referred directly back to the physician. He begins by relieving the pain, and not only does so, but tells the patient what he is using for that purpose. I cannot speak in regard to the neurotic diathesis, but some of the best developed, and apparently best balanced, patients that I have had, so far as external appearances were concerned, were addicted to the opium habit, and some of them are still addicted to it. If the physician is forced to give opium, he should give it secretly.

As has been said, opium, by its stupefying influence upon the nervous system, covers up and masks the symptoms. The physician also is lulled into a feeling of security, and does not recognize the destructive changes that are taking place, and is only awakened when he finds his patient absolutely dying. I have seen cases of appendicitis where the physician thought that the patient was improving, and yet the abdomen was full of pus and the patient dying. The opium had not only quieted the patient, but also lulled the doctor into a feeling of security. Not only in appendicitis, but in other surgical conditions, where the pain and discomfort is so great as to call for the use of opium, something should be done instead of giving opium, unless the patient is past relief; there opium is proper enough. In abdominal surgery we have come to the firm conviction that opium is uncalled for except where the opium habit has been established.

Dr. J. P. Crozer Griffith: I stand here to-night feeling somewhat alone, since I intend to take issue in some respects with what has already been said in the discussion, and to consider the matter from the standpoint of the physician,

rather than that of the surgeon. I agree fully with nearly all that was told us in the papers of the evening regarding the dangers of opium, and assent as well to the statements of Drs. Price and Hoffman, that in nearly every case of the opium habit the physician is probably responsible. At the same time, one cannot but be impressed with the feeling that one of the laity, strolling in here to-night, would leave with the thought that opium is an unmitigated evil, a drug which should never be employed under any circumstances whatever, inasmuch as it never did anything but harm. Every unprejudiced physician can only regard this as radically wrong. I say it after mature deliberation, and not on the spur of the moment, but still I say it, that were I compelled to select one drug from the entire list in the Pharmacopœia, and confine myself to its use alone, I would unhesitatingly choose opium. And though in this I am at variance with what we so often hear uttered in discussions in this Society, yet I trust I am not an antiquated fogy, and I know I try to keep abreast of the times. It has been urged that opium rarely cures disease, but only relieves symptoms, at the same time that it disguises them. I should like to inquire how many drugs in the Pharmacopœia really cure; certainly there are very few. As educated physicians, we hesitate to say that we have *cured* our patients. All that we expect to do is to guide our patients to recovery. I have no reference in this connection to the employment of surgical interference. Were it true that opium never had any powers but those of relief, there would, therefore, be no objection to its use on this score. Well aware of the dangers of the opium habit, I would hesitate long before beginning the employment of the drug in cases which were certain to run a somewhat chronic course, yet in which there was hope of recovery. Dujardin-Beaumetz has made the statement that thirty days' continuous use of opium will make an habitué of any one. In the light of what has been said in the latter of the two papers of the evening, this is perhaps an overstatement. It at least expresses the opinion of an excellent French authority.

There are cases in which the greatest fanatic will probably admit that it is our duty to administer opium in some form. Among these are hopeless cases of cancer. It is true that the drug gradually loses its effect, and that the dose must constantly be increased. But in no other way can existence be made even tolerable. I believe, too, that it is almost a sin to omit the employment of opium in the last stages of many cases of phthisis. The statement has been made in text-books that opium is poison to patients with phthisis. While this may be true in theory, it is eminently untrue in practice. Of course, I have no reference to phthisis in the early stage. There is no question that in this stage we should defer employing it, trying other measures for relief, and devoting our attention to the cure of the disease.

While it is true that opium may occasionally cause—perhaps it is better to say *be followed by*—suppression of urine, dyspnœa, and other unpleasant and dangerous symptoms, we should not totally abandon its employment on this account. I have a patient in whom five drops of tincture of nux vomica is always followed by unpleasant evidences of the physiological action of strychnine, but I shall not on that account refuse to give nux vomica in other cases. I have seen retention of urine follow the application of turpentine stupes, but shall not for that reason abandon their use. So numerous drug-idiosyncrasies exist that the man who inordinately fears them will never give anything, and had better retire from the profession.

As there are a number of gynecologists present, I am ready to admit at once that I understand very little about abdominal surgery. But as a physician it is my duty to have some opinions about peritonitis. In former years, before the diagnosis of pathological conditions of the abdomen had so degenerated that it became necessary to cut the patient open in order to discover what was the matter, there were physicians who possessed sufficient diagnostic acumen to recognize peritonitis without this interior inspection. Dr. Alonzo Clark was one of these. Unfortunately for him, he died before he had the full opportunity of learning from the later lights concerning the avoidance of opium in peritonitis and the proper treatment of the affection, and he claimed that in some way it was partially curative. It was his custom to administer opium in very large doses in this disease. Doubtless it was very stupid of him to persist in this practice, but strange to say his cases recovered. It may, after all, not appear so strange when we call to mind that the use of depletives in peritonitis, including laxatives in some cases, had been in vogue up to that time, and with results far inferior to those of Dr. Clark's. For my own part, I have yet to hear of any method of treatment which supersedes the administration of opium in this affection—not as a calmative agent, but as a curative one as well. Of course, there are cases in which every physician would counsel operation; but this is in no wise a sanction to indiscriminate laparotomy.

With regard to what has been said about opium in peritonitis beguiling the physician into a sense of false security, it is, of course, necessary that the attendant should have his wits about him, and realize that the drug is liable to disguise the symptoms by relieving pain. I think, however, that there are other factors than the sensation of pain to indicate the existence of danger-signals.

Then, again, we must not forget that it is often impossible to obtain consent for an operation, however desirable this procedure may appear. In such cases we wish to be familiar with some method of treatment other than the employment of the knife. We shall sometimes be surprised by the recovery under the administration of opium of cases which we had deemed hopeless without operation.

Dr. W. M. Capp: It is to be remembered that the physician is often hurriedly summoned to patients with acute symptoms and in great suffering, and it is from these that relief is sought. Generally speaking, it is well to say that we should sit down and study the case, to discover the cause of the suffering, and remove it, so that relief will naturally follow; but practically something more speedy is demanded, and a narcotic may be useful to tide the patient over what would otherwise be a critical period of suffering.

Dr. Crothers' paper was very interesting, but I do not know that we can agree with him in substituting the word disease for the word habit in all cases of opium addiction. I think that in many cases the use of opium is simply a habit, in no respect different from the alcohol or the tobacco habit. There are some persons with such weak will-power that they go through life, dominated by a habit of some kind. The remark that the physician is largely responsible for the prevalence of the *improper* use of opium is rather too general. It is too sweeping an assertion to say that the physicians are responsible in all cases. It has also been intimated that from the frequency with which opium is prescribed we can easily account for the large quantity which is imported. This is too broad a statement to eminate from a society of this kind. Let me refer to one single fact. It is generally known that almost all patent medicines contain opium, and that there is no country in which so large a quantity of these nostrums is consumed. In their manufacture a large quantity of this drug is used, and it is but a short step from the use of narcotic patent medicines to the direct use of opium. There are many ways other than by the prescriptions of physicians, by which people become addicted to the use of opium.

Dr. Edward Jackson: The power of opium to cover up symptoms has been mentioned. This is something more than the power to prevent pain by an action on the peripheral nerves or on the centres themselves. I have seen this illustrated in some cases of insufficiency of the eye muscles. I have one patient with hyperphoria of six centrads, who has suffered much from headache and other effects of the eye-strain. If she takes three-fourths of a grain of opium, the hyperphoria entirely disappears. She is not hysterical and is not an opium habitué; she goes many weeks and even months without a single dose. For several hours after taking the opium the conditions of nervous action are so radically changed that the hyperphoria no longer exists, so far as we can in any way discover it. My attention was called to a similar case by my friend, Dr. Charles H. Thomas. In these cases there is certainly some peculiar effect on the coördination of nerve impulses, so that both the pain and also its cause are for the time removed.

In regard to the lessened importation of opium, it occurred to me that this might be connected with the introduction of hyosine and some of the coal-tar derivatives, and their wide use by both the profession and laity; also with the attack led by the abdominal surgeons on the use of opium in what was formerly regarded as its peculiar province, the abdominal inflammations.

Dr. William H. Welch: A celebrated physician of this city once said he could count on the fingers of one hand all the drugs that he had found of real use in his practice, and he would place opium at the head of the list. Now I feel very much the same way. Of course, I recognize the fact that opium, like alcohol, is greatly abused; but I would not be willing to give up either on that account. I have met with a number of persons who have become habituated to the use of opium, but I have seen only one case in which I have aided in breaking off the use of the drug. This patient was a nurse,

who had contracted the habit voluntarily. He began the use of morphine to induce sleep, after having met with a bereavement in his family. I think he also suffered from neuralgia. He gradually increased the dose up to two grains a day. After continuing this for two years he decided to give it up, and so he gradually diminished the daily dose for four days, when he discontinued it entirely. To relieve the pain in his extremities, he took antipyrine and also bromide of sodium to promote sleep. After abstaining from its use for several months he went on a visit to New York, and while there was taken with cholera morbus. He applied to the nearest drug-store for relief, and the druggist gave him a mixture containing opium in some form. From this time he returned to the use of morphine, and soon reached a daily dose of four grains. After continuing the use of the drug now for two years longer, he again resolved to break off the habit, and this time placed himself under my care. It is unnecessary to recite in detail the symptoms that followed, but the extreme muscular weakness was very surprising, especially when we consider that the daily dose was only four grains. In a few days after discontinuing the drug he became absolutely helpless; he could not feed himself, and could not hold in his hand even a glass of water. He became delirious. In the course of two weeks he began to improve, but it was three weeks before he was able to resume his duties. I should add that he took bromide of sodium very freely, and some critics have suggested that the extreme muscular weakness may have been due to this drug. He, however, showed the weakness before the bromide was taken. He took as much as two hundred and forty grains of the bromide daily for several days. I know nothing of his family history, but so far as the individual himself is concerned, there appears to be no neurotic element in the case. It has now been two years since he stopped the drug, and he tells me he has not returned to its use. But these people often practice so much deception that I cannot be absolutely sure on this point.

Dr. T. B. Schneidemann: I recall a valuable little book, now out of print, by Professor Fiske, of Harvard, on alcohol and tobacco. The title-page bears the legends, "1. It Does Pay to Smoke." "2. The Coming Man will Drink Wine." The author is at some pains to define the difference between a stimulant dose and a narcotic dose, the former being such an amount as nourishes or facilitates the normal nutrition of the nervous system, restoring its equilibrium, enabling it with diminished effort to discharge its natural functions. Such a dose, moreover, has no evil after-effect—"reaction"—which belongs only to a narcotic dose, the term over-stimulation being a misnomer, and imported into physiology from *à priori* reasoning. As a stimulant, he declares that these substances diminish the friction of life, and are useful adjuncts to civilization, and that the dose does not have to be increased, and that their use may be dispensed with at any time without exciting morbid craving. Fiske's views are essentially those of Anstie.

Dr. T. Ridgway Barker: There is one point in Dr. Crothers' paper that impressed me, and that is the importance of hereditary predisposition. This belongs to what is the interchangeable neuroses. If the histories of cases are more carefully examined, we shall find that inherited tendencies have a great deal to do with the use of narcotics and stimulants.

I think the physician should not wholly escape from the charge of too frequently prescribing morphine and other narcotics. It is not uncommon in cases associated with pain for the physician to take from his hypodermatic case tablets of morphine, and instruct the patient to take one every hour or so, according to circumstances. The patient is aware of the nature of the drug and may acquire the habit.

In cases of post-partum hæmorrhage one-drachm doses of tincture of opium certainly have a happy effect. Even where the patient is almost exsanguined, she may recover.

I was glad to hear the recommendation of Dr. Crothers in regard to the gradual diminution of the dose, thus disturbing less markedly the nervous system.

Dr. Hoffman: Very little weight can be attached to the recommendation or argument that certain men used opium twenty or twenty-five years ago with good results. There is no doubt that in certain acute painful diseases opium is indicated, as in rheumatism, the passage of a stone, and in neuralgia. When the surgeon talks about peritonitis, he talks about something that he knows, while the general practitioner talks about something he imagines. Nine-tenths of the cases of so-called peritonitis are not peritonitis at all. When the abdominal surgeon opens the abdomen, he makes a peritonitis, for the reparation is due to inflammation. In these cases, no matter how great the swelling and pain, it passes away under the use of salines or calomel. I think that here we have the vantage ground of the argument, for we know what we have done, and we have the results.

Dr. M. Price: I should like to call attention to the statement in regard to the general practitioner and peritonitis. When the physician gets a case of peritonitis that he thinks is going to die, he refers it to the surgeon. The very men who are boasting of their twenty or thirty cures of peritonitis are the men who are referring to the surgeon their fatal cases with quarts of pus in the abdomen. When we cure from 93 to 97 per cent. of the cases that they give up, I think that we have the best of the argument. A case was recently reported from Buffalo of a girl ten years of age treated with morphine for fourteen days, the daily dose finally reaching twenty-five grains. The temperature had disappeared five days before this dose was reached. They then gave her a saline; they had given salines before, but the child vomited it. I cannot understand such peritonitis. Here must be some neuralgic or nervous condition mistaken for peritonitis.

Dr. Werner: The remarks of Dr. Crothers seem to reinforce my position, that when indicated opium should be given with great care, and in such a way that the patient does not know that he is taking opium. Some years ago I was called to see a child one month old. I found it in convulsions, with contracted pupils; the child had previously been well. I asked if they had given it any medicine. They at first denied that they had, but subsequently admitted that a neighbor had given the child a dose of medicine for colic. The child died in less than twenty-four hours, and subsequent investigation showed that it had received an eighth of a grain of morphine.

Another case that I recall is that of a young girl of strumous diathesis, who had been treated for various troubles, and finally, going to a gynecologist, a stem pessary was introduced. She came to me, I removed the pessary, and found the uterus and broad ligaments swollen and extremely sensitive; there was menstrual pain. She was placed at rest, with suitable treatment, and improved. Being a working girl, she is compelled to be much on her feet, and soon all the old pains returned. I told her that if she could rest she might be made comfortable, but that otherwise it would be necessary to remove the diseased appendages. She refused operation, and the last time I saw her she told me that a friend promised to give her some powders, which she thought contained morphine. I explained the dangers to her, but in spite of that she is taking the morphine. I do not know what the result will be.

These cases are sufficient to show the necessity of calling a halt. Prescriptions should not be allowed to be renewed at random. The cases for the use of opium are rare, and should be carefully selected, and the physician should give the dose himself.

I fully agree with the remarks made by Drs. Price and Hoffman regarding the use of opium in abdominal work, and feel certain that many lives are saved by having given up its use; it has certainly often lulled both patient and physician into a dangerous feeling of security until it was too late. I recall one case of septic peritonitis which I saw in consultation. Although the patient was comfortable after calomel and salines, I knew from the pulse and temperature that there was pus, and suggested operation. The family physician refused operation unless I could show him that pus was present. I agreed to remain in attendance, provided opium was not given. On Wednesday he gave the first dose of opium, for what reason I do not know; there was no pain, and there were daily movements. After the opium, distention of the abdomen occurred, and death the following Sunday. In these days, when we are trying to have cleanliness in everything, why should we shrink from giving salines and cleaning the bowel of any foul material? Why should we block the bowels with opium when we know that the products are reabsorbed? These are questions which I should be glad to have answered from the other side.

Dr. Crothers, of Hartford, Conn.: The interest which this subject has excited is very pleasing to me. The point which I tried to make clear is that these are neurotic cases, either acquired or inherited, and that we should treat them as such, and bear this fact in mind in our use of opium. If we have a neurotic case, we should remember this predilection to the opium habit.

Dr. Mays was among the pioneers who exemplified the close connection between alcoholism and consumption, and between opium-taking and consumption. Phthisis, alcohol-

ism, opium-taking, and a host of other affections, are neurotic conditions, and interchangeable one with the other.

I have strongly objected to the word habit, because its exact meaning is not understood. We use the word in its common sense, and not in its scientific sense.

There are many reasons for the discrepancy between the quantity of opium imported and the quantity used legitimately. It shows that opium-taking is increasing, and that opium is used in various secretive ways.

I think that there are no physicians in general practice who will regard opium as a bad remedy. I think that the statement that it is one of our chief remedies will be confirmed by all. It is, however, clear that it should be given with great caution, and its use concealed from the patient's knowledge. We cannot dispense with it under any circumstances. My principal point has been to call attention to the neurotic character of these cases, and not to disparage the use of opium.

With regard to the treatment of opium cases, we come to a wide field which the general practitioner is not quite ready to enter. The routine treatment of diminishing the quantity of opium as the only plan of cure employed must be abandoned. Opium must be retained or diminished according to the case. In some cases it would shorten life if the opium were diminished. In the large proportion of cases, however, the opium can be removed. The routine plan of diminishing the opium gradually or rapidly is largely empirical. No one should undertake the opium treatment without knowing the diathesis and condition of his case, and then the question of the removal or diminution of the opium becomes clear, and the result satisfactory.

SOCIETY PROCEEDINGS.

American Electro-Therapeutic Association.

First Annual Meeting of the American Electro-therapeutic Association, held in Philadelphia, September 24, 25 and 26, 1891.

(*Continued from page 201.*)

Dr. Von Raitz: I have nothing to say about treating the stomach with electricity. Einhorn uses about the same apparatus as Dr. Wolff, but uses a current with slow interruption, and as far as washing out is concerned, he does that before. If a person does not retain his food very well there is irritation of the stomach, and he has to vomit, and then he considers there is inflammation of the stomach. He uses galvanism and the negative pole. In another class of cases he gives food at night and prevents hydrochloric acid from attacking the stomach at night.

Dr. Morton: These two papers need more discussion than we can possibly give them to-day, but we appreciate them greatly. I have used about the same tubes making different attachments, first one and then another, and I like Dr. Stockton's very much.

I think more attention in both papers might be given to the exact character of the current they have been using. Dr. Stockton says he applied both the continuous and interrupted or faradic current. There is a vast range between the interrupted galvanic current and the faradic, and I would have been glad to have had something specific in that respect, and I shall read their papers carefully so as to get at the exact current they have been using.

Dr. Massey: These papers are extremely interesting to me. In 1886, in an idle moment I conceived it would be possible to get up an electrode like a pill so that it could be fastened to a string and swallowed by the patient, and I had Mr. Fleming make me one, which I show to you. The bulb consists of a rubber cage to hold absorbent cotton, and the cord terminates inside with a metallic end. In handling it, I found that a safe attachment was important, and that a knot was better than a metallic fastener. I have, unfortunately, never been able to get any one to swallow it, and after reading the valuable remarks of Dr. Stockton in a medical journal some time ago, I took it out of the drawer and examined it. I had two sizes, as you see, and they represent a method of making a movable electrode for the stomach, if you can get any one to swallow them. I tried it on a patient recently, and on his refusal to swallow, made it stiff by putting a tubing over it, by this means getting it down, but I could not keep it there, as the patient was not accustomed to irritating substances or lavage. I think this apparatus is theoretically beautiful, but it will have to pale after the more practical apparatus shown by the doctor.

Dr. Goelet: I desire to ask a question as to the necessity for so much stress being put upon the insulation for the faradic current, as it seems to me any metallic surface would do, as it would be distributed better in the stomach. Of course, in galvanism insulation would be necessary.

Dr. Stockton: I feel tempted to say a great deal more than perhaps I should, as I have left a great many things unsaid. I did not put my remarks in even, but it will be impossible for me to correct that now, and I will ask the gentlemen to read my paper which has been given to the secretary.

Dr. Wolff's paper and my own agree. I was prepared for this, for I have read the doctor's papers on this subject. Before discussing the doctor's paper, I would like to answer in regard to the currents employed. First, I always advise the use of the faradic current in conditions of diminished peristalsis, with the exception of those cases previously spoken of, and in a strength sufficient to produce contraction when it does not give pain, and there is a valuable point as to the insulation with rubber. It is found impossible to get sufficient contraction with metal uncovered even with the faradic current without great pain, and provided the current is diffused through the stomach, you can do so without much pain, and I have proven this many times by pushing the wire down too far, and having got a spasm of pain in the stomach if the stomach contracts directly on the tip. As to the continuous current, I more often employ it in other conditions than that of mere failure of peristalsis. I have had a somewhat different experience from that of Dr. Wolff as regards the use of the continuous current in the treatment of neuralgias. In true neuralgias I get great benefit from the continuous current, and I use the anode internally with the strength never over 20 milliampères, and 10 milliampères is usually sufficient with large sponge electrodes externally, well wet with saline solutions, and I have seen great benefit where there is deficient digestive strength, as I have proved by examination. I find good results after using the continuous current, and often with the faradic, but not so satisfactory. After introducing the electrode and applying the continuous current, and then removing the alkaline fluid put in and then put in water, you will find hydrochloric acid. I think there is no doubt about this, as I have made the experiment many times.

In speaking of Dr. Wolff's paper, I constructed an electrode like his in 1887, and discontinued it for the reason that it required the removal of the stomach tube; otherwise it is quite as satisfactory, as you can apply the current the same; but as it seems best to remove the contents of the stomach first and the saline fluid afterwards, and to study the contents of the stomach, my apparatus avoids the removal of the tubes so often.

The president's experience is new to me. I never had any experience that way. I have used the saline fluid in the stomach tube, and used a wire through it, and then I gradually led up to this instrument which I show you, and it has proved very satisfactory to me. I have constructed one five feet long, doing away with the clamp, and you can introduce that, but it takes a long time, and it is not necessary.

Dr. Wolff: In reply to Dr. Massey, I see an objection to the mandrel, that it might be left behind. I do not know how to remedy it, but this mishap has never befallen me.

In answer to Dr. Stockton, I would say that I can confirm almost everything that he has said with the exception of the use of galvanism as applied to the stomach. When I said I could not detect any increase of hydrochloric acid, it was by the external use of a galvanic current, and I still believe that the galvanic poles, externally, do not influence in any way the secretion of the glands, but when applied internally they will produce a greater secretion of the peptic glands. I think the direct stimulation by galvanism or faradism will stimulate them to a greater degree than anything else. I have never used saline solutions; they are disagreeable to take, and they produce an inclination to emesis, which is also done by electric stimulation. There is a great deal of hydrochloric acid thrown out, and as there are no better conductors than acids, we do not need to add salines.

As to the use of the faradic current, we fully agree as to its great utility where atony of the muscular fibers of the stomach is found. I thank the gentlemen very much for their kind consideration.

AFTERNOON SESSION—September 26, 1891.

Meeting called to order at 3 P.M., with President Massey in the chair.

Dr. Bigelow: I move that the Executive Council have the transactions prepared and printed, and sent to all the medical journals desiring them, and that the permanent form of the proceedings be preserved for printing when necessary, and that the members look after the publication of their own papers.

Seconded, carried, and so ordered.

Dr. Morton: In behalf of the visiting members, strangers to you, I arise to thank you for your cordial greeting. We go home feeling a new impetus to our studies and to our work, and we owe a great deal of it to the manner in which you have promoted this Association, in this, its first meeting. The birth of every new enterprise has always a certain interest, and the beginnings and interest of this meeting will probably never leave the minds of the participants. As it is the first meeting, I say, Mr. President and brother physicians, we thank you collectively, we thank you individually, we thank you with warm hearts for the cordial and kind manner in which you have greeted us, and much of the success is due to the manner of presiding and the President's smooth working; and I thank the Secretary for his hard and quiet labor, often into the night hours; and I thank Dr. Bigelow, for to his personal efforts are due much of the success we have had. Thanking you, Mr. President and the people of Philadelphia, in the name of the strangers present, we go home with a feeling of warm gratitude, and I move that a vote of thanks be incorporated.

So ordered.

Dr. Massey: I am sure that we all receive the words which have been spoken with much feeling.

Horatio R. Bigelow, M.D., read a paper entitled

ALTERNATIVE CURRENTS.

Mons. D'Arsonval, Professor of the College of France and Vice-President of the Societi Francaise D'Electrotherapie, has presented two ingenious and scholarly papers to this society, which will be found in the May and June numbers of the *Revue Internationale D'Electrotherapie*, and which I propose reviewing in thesis. The physiological effects (excitation of nerves and muscles) which are obtained from electricity seem to be, *a priori*, very different according to the service generating the current. These differences, according to Mons. D'Arsonval, are due simply to the physical form of the electric wave, or the characteristic of the excitation. This question was discussed at the International Commission of Electro-Physiology in 1881, the Professor being the Secretary of the Commission. The members were Messieurs D'Arsonval, E. DuBois Reymond, Christiain, Gariel, Helmholtz, Tonbert, Zipmann, Marcel, Deprez, Moscart, Rossetti, Terquem, von Ziemssen. A few days after the adjournment of the commission, M. D'Arsonval believed that he had invented a machine capable of registering automatically the curve of excitation. This being submitted to Helmholtz and DuBois Reymond, they replied that there could be no theoretic objection to it, and that it should be given a trial. This method allows us to trace out upon a special cylinder the curve or characteristic of excitation, and just below it, on the same cylinder, the muscular contraction provoked by the excitation. The characteristic of excitation is a function of three variants—the *potential*, the *time*, and the *quantity* of *electricity*. The curve might be represented by carrying the time upon the axis of x, and the potential upon that of y. The curve would then give the variations of the potential in function of time, which is the chief element in the study of electric excitation. The quantity of electricity is determined, either from a knowledge of the total resistance of the circuit, or by the interposition in the circuit of a condenser of a known capacity. The salient feature in the process of M. D'Arsonval is the employment of a liquid resistance on the one hand, and on the other a variation of potential by the mechanical displacement of a conductor moving in the column of liquid. In this way the variation of potential is continuous, and the law of this variation is identically the same as that of the movement. In modifying the movement we modify the variation of potential, or the characteristic of excitation. Not only is the form of the curve known to us at each moment, but we can modify this form at will. M. D'Arsonval devised an instrument as follows: A constant pile with a homogenous liquid (sulphate of copper or mercury); one of the poles of the pile is grounded; we can then vary the potential from zero to maximum at will; a conductor insulated to a certain distance is so attached that it can rise and fall in the liquid column, and can thus also vary its potential from zero to maximum. The potential of the conductor is proportional to its distance from the bottom of the liquid. This conductor is attached to a movable lever; the other extremity of the lever traces the curve upon a cylinder, which is the variation of potential of the plunges in the liquid. By bringing a portion of a nerve into the circuit the muscle supplied by it may be excited by moving the plunges, and its contractions will also be inscribed upon the cylinder, so that we have two tracings, one showing the characteristics of excitation, the *cause*, the other giving the muscular contraction, or the *effect*. A condenser may be interposed, or the primary wire of an inductive coil, the secondary current being used to excite the muscle. In this case the muscle will receive electricity during the movement of the plunges only. By attaching the plunger to the arm of an electro-diapason we have a variation of current known as sirrus-sördale. To get the alternating current it is only necessary to intercalate an induction coil. These alternating currents have the immense advantage of not fatiguing the part to which applied. A nerve is extremely sensitive to the rapidity of the variations of potential, and the smaller the angle which the origin of the curve or characteristic makes with the axis of y, and the less spread out that the characteristic is upon the axis of x, the greater will be the excitation. For muscles, just the reverse obtains, the muscle being a better conductor than the nerve. In the muscle, as in the nerve, it is the change of potential between points which seems to be the chief cause of excitation, the only difference being that the interruption must be more rapid for the nerve than for the muscle. So it happens that in prolonging the duration of a discharge we may reach a point

where the nerve is scarcely excited at all, while on the other hand the muscle is strongly agitated.

This is why the "break" of an inductive current excites the nerve, while the closing of the current of the pile (a much longer discharge) excites the muscle. This being so, in prolonging the duration of the current of induction, in order to have quantity, we can obtain effects exactly similar to those of the pile. This can be demonstrated as follows: Take an inductive coil of coarse wire and mobile helices (such as that of Tripier), worked by two of Gaiffe's chloride of silver cells with moistened tampons, excite the muscles of the thenar eminence; the contraction is strong but painful. The sensory and motor nerves are influenced at the same time with the muscles without changing anything; intercalate on the induction wire a condenser of one microfad; the muscle contracts as before, but there is no pain. What has taken place? Simply this: the interposition of the condenser has delayed the discharge, without modification of the quantity, as is shown by the balastic galvanometer.

Indeed, the condition of the induced discharge is proportional to the product of CxR, of the capacity, C, by the resistance, R, of the system. The resistance is the same in both instances, but it is not so with the capacity. It is about one-thousandth of a microfad only before the interposition of the condenser. After this, however, it is nearly a thousand times as great. We have all the intermediate varieties. In interposing successively $\frac{1}{10}$, $\frac{2}{10}$, etc., xxx $\frac{10}{10}$ of a microfad, we have gradually, with the same machine, physiological effects, currents of induction similar to those given by the pile. If we measure with a balastic galvanometer the quantity of electricity given by a discharge from the condenser, charged by means of the coil, we find 2,200 microfads, which is equal to the amount of electricity which 2,200 chloride of silver couples would furnish. Masson was the first to employ this arrangement, in 1854, to intensify the calorific power of the induced spark. It seems to me that this procedure will be of the greatest service in causing painless excitation of the muscular system (as Tripier treats uterine deviations, etc.), and also will it be of service in studying the reactions in the degeneration of Erb. We come now to a physiological study of the action of alternating currents. Mons. D'Arsonval read a communication before the Biological Society last winter, giving his experiments to determine whether the different methods of electrization could modify the respiratory gaseous charges and the thermogenesis. He found that the static bath augmented the absorption of oxygen, and the elimination of carbonic acid. Nothing remarkable happened with the galvanic current. The induced currents were accompanied with great muscular contractions, which greatly modified these changes. He then passed through the organism alternating currents generated by a Siemens bobbin, the period being long enough to occasion very slight muscular contraction. Under these conditions, the passage of these currents, without any motor reaction, augmented considerably these changes. Although not accompanied with any appreciable electrolytic effects, they are very liable to produce the trophic changes which, at present, are expected from the galvanic current; effects generally attributed to electrolysis, when brought to bear upon certain tumors that are in the intra-polar circuit (fibrina, etc., as practiced by Tripier and Apostoli). There are two methods suggested by Mons. D'Arsonval. One is to make use of a magnetic machine as a generator of alternating currents, intercalating a suitable ampère meter, giving the least intensity in milliampères, as for the galvanic current; the other consists in making use of the ordinary galvanic cell, and then generating these currents by means of a rotating commutator of a special form. The machines in ordinary use are characterized by great differences in their physiological effects. These differences are all due to the inequality of each electric wave produced by these various machines. Mons. D'Arsonval invented a most ingenious appliance, an optic galvanograph, for tracing these curves. Clarke's machine was found to give a curve indicating shocks of some severity. In this machine the line is abrupt and broken; in the sirrus-ördal machine, the line is harmonious, even and unbroken. The intensity of these currents was measured with an aperiodic balance of Curie, on the beam of which was suspended a small loop of very fine wire, that entered a fixed bobbin through which the current passed. Such a machine, for generating these sirrus-ördal currents, has been made by Gaiffe. He constructed a collector capable of collecting the currents of the pile, and by means of a gradual augmentation or diminution of the current, followed by reversing, sirrus-ördal currents are generated. This collector is composed of two concentric metallic discs, mounted on a wooden box, one of which, the most external, is divided into 114 segments, insulated the one from the other, and capable of being brought into communication with a battery of 28 elements. The internal disc is divided into two segments, which are also insulated from each other. Upon each disc is fastened, for gentle rubbing, a metallic brush, the revolutions of which can be governed at will. These brushes are supported by a crank divided into two separate conductors, one corresponding to the external brush, and the other to the internal brush. This crank rests on an axis, which is also divided into two conductors, and against which two other brushes rub, which transmit the current collected to the binding posts of the current taken, which are in front of the machine. This disposition allows us to collect at the binding posts of the active machine, the current of either the internal or external disc.

The elements of the pile are coupled in tension, and united one by one to the 29 first segments of the external metallic disc, all the carbons being placed in communication with these segments, which are consequently positive. The negative of the first element to the half internal disc, corresponding to it. The twenty-eight segments following the external disc are united one by one to the first twenty-eight segments. In a word the external brush rubs successively the segments 1, 2, 3, 4, etc., up to 29; it continues to advance meeting segments 28, 27, 26, etc., down to 0, while the internal brush becomes exhausted upon the un-interrupted half disc, giving a negative current. At zero the brushes find a condition just the reverse of this, the segments of the external disc are there united to the negatives of the battery one by one, and the positive is united to the internal half disc. From this disposition we have a regularly alternating current, from maximum to zero. The brush movement can be obtained by a toothed wheel, using the hand to rotate it, or by motor. A theostat can be intercalated to regulate the current.

We all know that Clarke's machine is purely one of magnetic induction. It consists of a fixed magnet. Two bobbins, wound with fine wire, the one right handed, and the other left, which are revolved by an endless loop of tube attached to a crank, and a very ingenious commutator so arranged that these solonoidal actions of alternate half revolutions in opposite directions, may be modified. This machine is not without much merit, but the characteristic of excitation shows great irregularity, and much shock. The Slatic induced current is very pleasant, very soothing and superior, so far as my own observation goes, to the induced current of the coil. In the sirrus-ördal current, however, we have something entirely different. A current capable, I believe, of giving startling results. I am sorry that my battery from Gaiffe has not arrived, that I might demonstrate it to you. Apostoli is just now experimenting with it in uterine tumors, and I await his results with great interest.

Dr. Morton: I do not think on a paper so very technical in its character that there is much to to be said until we can study out the details which have been presented in but a brief resumè. I feel myself in doubt as to the construction of the coil. I saw the article but late on Friday night and have not had time to read it yet. I gather in a general way that it resembles the undulatory current of the Clark coil and that it resembles the Clark machine, and I think it is better to think of it that way for the time being. We are thankful for this current intensity and potentiality pointed out to us in its relations to particular diseases. In the induction coils we have a distinct interruption in the primary circuit, and in the secondary circuit we have the undulatory current as in this case. The advantages claimed for this, as I understand it, depend upon the continuity of the current. It is a curious thing that this very principle has been the means of amassing millions of dollars in the great Bell Telephone Co. Between the Rice telephone and the Bell there is this difference: In Bell's the circuit is continuous, which is not in the other, and the two electrodes of the Bell's are always in contact, sometimes light, sometimes heavy; so that that vast monopoly has been built up just exactly upon this little plain thing of D'Arsonval's improvement, namely, continuity and uninterrupted current; and if this can make a difference of millions of dollars in the industries, it may make a considerable difference to us in medical treatment. I shall await the trial of this new current with considerable anticipation.

Dr. Bigelow: There will be a considerable advantage in this, for you can record the wave of sedation of every patient in the clinic.

J. H. Kellogg, M.D., read a paper on

TWO NEW ELECTRODES.

I wish to bring before the Society two new forms of electrodes for therapeutic use, which I have found of practical value and have used quite extensively within the last year.

The first of these is a flexible, slightly adhesive electrode which is used dry. The electrode is composed of gelatine, finely powdered graphite, glycerine, and chloride of sodium. It is made as follows: Dissolve 20 ounces of the best gelatine in 10 ounces of boiling water; add 10 ounces of glycerine and 2 drams of sodium chloride; heat and add 10 ounces of finely-pulverized gas carbon, mixing thoroughly.

To form the above mixture into an electrode, take a shallow tin pan of the size desired for the electrode. Oil the inside of the pan with vaseline. Pour in a sufficient amount of the hot mixture to cover the bottom of the pan; lay in the pan a piece of sheet lint, cut of sufficient size to allow the edges to turn up about one-half inch around the sides of the pan; pour in some of the mixture, sufficient to saturate and cover the lint; lay in another piece of the lint, a little smaller than the first, and cover this also with the mixture in the same way. A third and fourth sheet of lint may be added, if necessary. Usually, two pieces are sufficient to give the desired strength. A piece of brass-wire cloth, to one corner of which a binding post has been attached, is next laid in; add more of the mixture, if necessary, and then another piece of lint. The wire cloth and the last layer of lint may be a trifle smaller than the electrode is desired to be. Lastly, fold the upturned edges of the first layer of lint over the back of the electrode, and apply a sufficient amount of the mixture to bind them in place. When the electrode is cold, and sufficiently hardened, carefully remove from the mold. If the surface of the electrode is not perfectly smooth, it may be polished with a hot spatula. Whenever the surface of the electrode becomes roughened by use, it may be smoothed in the same way. If the electrode becomes cracked, or its surface very irregular, it may easily be repaired by applying a little of the hot gelatine mixture, and smoothing with a spatula.

This electrode is light, clean, adhesive, a good conductor of electricity, and durable. I have often had an electrode of this sort in daily use in my office for weeks without being able to detect any material deterioration in it. I find that a greater quantity of electricity can be communicated to the patient through an electrode of this composition than through a clay electrode of the same size. I attribute this to the more perfect contact between the skin and the gelatine-graphite electrode than is obtainable with a clay electrode.

I first began experiments with reference to the construction of this electrode in July, 1890; I began the use of the electrode in August of the same year, and have employed a number of them in my office since that time. From twenty to fifty applications of the galvanic current are made in my office daily, in a large proportion of which currents varying from 40 to 250 milliampères are employed. I have constantly under treatment a considerable number of cases of myoma, but I have not, since I began the use of this electrode, found it necessary to resort to the clay, or any other form of electrode in a single instance. A flexible and adhesive electrode may be made by combining red lead with the gelatine, and other substances capable of acting as good conductors.

I have recently had constructed and have used with much satisfaction another form of electrode, which so far as my knowledge extends, is novel. It consists of a metallic vacuum cup, to which a binding post is attached, a moist sponge being placed inside. In use, the air is passed from the cup, causing it to adhere firmly in position, and at the same time bringing the tissues in close contact with the sponge. In the use of this electrode three advantages are gained:

1. The vacuum created in the cup establishes a condition favorable to electrical conduction. The resistance of the skin is considerably lessened by the increased vascularity induced by the vacuum created within the cup.

2. Very excellent contact is produced by the strong pressure of the moist sponge against the tissues, resulting from the exhaustion of air from the cup.

3. An intensified impression is produced upon the tissues by the combined influence of the electrical current and the cupping.

I have found this electrode very efficient in the treatment of obstinate, deep-seated pains which had resisted the long continued use of both cupping and the galvanic current employed separately. I am still studying the properties of these electrodes, and shall be able to make, at a future time, a more precise report of their value and properties.

(To be Continued.)

THE SECRETION OF ACID IN THE STOMACH IN MENTAL AND NERVOUS DISEASES.—Observations on this subject have been made by Dr. Lenbuscher. He determined the acid present in the stomach two to three hours after meals. In 12 cases of melancholia, the condition was normal in 50 per cent. In 5 cases of mania, the acid was increased. In chronic paranoia, the proportions were normal. In 50 cases of paralysis [general paralysis?] examined, the proportions were, generally speaking, not normal, being so in 2 cases only; 2 showed no hydrochloric acid, 5 hyperacidity, and 9 had very little. In most of the cases, the quantity oscillated between absence and a high proportion. Paralytic cases showed diminution of acid, as also apoplectic and epileptic cases. In chronic morphinism in which the morphine had been withheld, the quantity was diminished. The treatment of psychoses with opium had no effect on the secretion. In chronic alcoholism the proportion was normal. In neurasthenia the quantity was generally increased. Patients much excited showed slight increase.—*Medical Press and Circular.*

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SATURDAY, FEBRUARY 20, 1892.

ANTIPYRETIC TREATMENT OF TYPHOID FEVER.

The remarkable results obtained by LIEBERMEISTER in the use of cold baths in typhoid fever at once attracted the attention of the physicians of the world. But his indiscriminate use of the baths in all cases in which the temperature became elevated, was, and is to-day much criticized. It is quite apparent that in such a disease as typhoid fever, there must always be many other factors to be considered than the single one of the temperature range. In America and in England the rigid application of the LIEBERMEISTER plan never found a very wide application. Social conditions as well as the judgment of physicians would not permit its use generally. In other fevers high temperature has not been found to be of the great danger which it has long been held to have in typhoid fever. But the long duration of the elevated temperature of typhoid is a factor which compels attention to the temperature in this particular fever. There has recently developed a school of physicians who affect to despise the thermometric showings in typhoid fever, and who never interfere with the temperature unless it reaches a very high degree, such as 105° or more, and even then unless the temperature remains for some time at this high point. But this ignoring of high temperature is probably more dangerous to patients generally than the routine employment of cold baths would be.

While a single upward spurt of temperature, is probably not dangerous in itself, it does not follow that long continued high temperature is equally innocuous. Recent experiments which have been made with microörganisms show that continuous exposure of them to a temperature of 104° or more, results in such changes in their chemical composition, that is to say, in their nutrition, as to modify greatly their reactions with staining reagents. If this be true of vegetable cells, and direct experiments prove it, it seems probable that it would likewise be true of the more delicate animal cells which go to make up the human organism. Definite experiments are lacking to prove this latter proposition, but the clinical results obtained by LIEBERMEISTER can admit of no other explanation than that continuous high temperature is destructive to the animal organism as a whole, and that systematic reduction of high temperature in typhoid fever is followed by good results.

But it must not be forgotten that elevation of temperature is only one of the elements of fever. The nervous manifestations of typhoid are parts of the fever to be considered apart from the mere matter of temperature. The degree of mental hebetude, the delirium, the muscular tremulousness, the weakness of the pulse, are all elements which are not to be measured by the degree of temperature present, and which yet are essential features of the fever, and in considering the results of the treatment by cold the effect upon these nervous manifestations must be noted. Now the administration of the cold bath according to the plan of LIEBERMEISTER whenever the temperature rises above 103°, unquestionably produces not only a lower temperature, but also affects most favorably the delirium, the tremulousness, the pulse and the mental hebetude. But as LIEBERMEISTER himself has shown, intestinal hæmorrhage occurs more frequently after its use, and relapses are more common.

The chemical antipyretics likewise produce a reduction of temperature, and with it, improvement in many nervous symptoms; but it seems that this is done at the expense of the general strength, and the idea is growing that the habitual use of the chemical antipyretics in typhoid fever, very materially prolongs convalescence, and in many instances impairs the strength to so great a degree that death ultimately follows. The judicious occasional use of chemical antipyretics in typhoid fever may be allowable, but their frequent or continuous use is disastrous, and in no way to be compared with the cold baths.

But to put a patient with a weak pulse into a cold bath is by no means free from danger. Fortunately, DR. JAMES BARR,[1] of Liverpool, has devised a plan of antipyretic treatment of typhoid fever, by water, which seems to combine all the advantages of the cold baths, while avoiding their objectionable features. DR. BARR'S plan is to immerse the patient continuously in a tank of water. The tank is 6 feet long, 2 feet 6 inches wide, and 16 inches deep. It is provided with a frame and straps of bed-ticking, for raising and lowering the patient into the water. The permanent tanks which he uses in his hospital are provided with open-mouth soil pipes to receive the discharges of the patients. The patient is wrapped in a blanket and completely immersed in water, ex-

[1] The Treatment of Typhoid Fever and Reports of Fifty-five Consecutive Cases, with Only One Death, by James Barr, M.D. London: H. K. Lewis, 1892.

cept the head, which is supported on an air pillow. The temperature of the water is raised or lowered according to the temperature of the patient. As the temperature of the patient approaches normal, the temperature of the tank water is made to approach this point, and as the patient's temperature rises, the temperature of the tank water is lowered. DR. BARR has not found it necessary to lower the temperature of the tank water below 90°, nor raise it above 98°. DR. BARR has treated altogether twenty-two cases in the tank. All of these have been severe cases. The patient is removed once each day to allow the tank to be cleaned. The duration of the immersion has varied from six days to thirty-one days.

The method is by no means an æsthetic one, but seems to be capable of producing excellent results. The inconveniences and difficulties which it entails are not to be considered in its use, if it successfully reaches a severe class of cases which it would be dangerous to treat by the wet pack or cold bath plan.

Space will not permit a review of individual cases, but DR. BARR'S general statements as to the effects of tank treatment, should receive careful attention.

Temperature.—No attempt was made to prevent the evening exacerbation of temperature, but merely to moderate it, and to secure as far as possible a daily remission to 99° or 100°. This was usually accomplished; in one case, however, the temperature remained persistently high, and occasional ice water enemata were given. DR. BARR believes that the action of this tank is truly antipyretic, the thermogenesis being diminished, the thermolysis being regulated, and the thermotaxic mechanism being improved.

Circulation.—The vaso-motor tone was markedly improved; the blood vessels became smaller and firmer; the pulse became slower, fuller and of improved tension; the heart maintained its vigor.

Respiration.—The respiration lessened in frequency; bronchitis and congestion of the lungs improved and soon disappeared. In one case, double pneumonia was present as a complication, the patient was in the tank nineteen days, and was cured.

Digestion.—The appetite and digestion improve, the tongue becomes moist and clean, the diarrhœa diminishes, and the stools improve in character.

Nervous System.—The delirium disappears and the general well-being of the patient greatly improves.

Weight.—Diminution in weight occurs, but not as markedly as usual.

Skin.—The horny layers of the palms of the hands and soles of the feet get quite macerated, but on the skin of the body generally there is very little effect, with the exception of a slight roughness and elevation of the papillæ.

The use of the tank does not interfere in any way with other treatment. The diet is to be regulated as usual, and intestinal antiseptics may be used if desired. Special symptoms are combated as usual.

THE DIAGNOSIS OF THORACIC TUMORS.

At the November meeting of the Berlin Medical Society, PROFESSOR VIRCHOW made some interesting observations on the diagnosis of tumors of the chest. He opened by saying that a helpful discrimination may be made at the outset by inquiring whether the tumor to be investigated is of the class which tends to ulceration, or to those that do not readily ulcerate. This being determined renders classification easier. To those tumors that do not tend to ulcerate, he has found, belonged the largest masses, and those that wrought the most extensive changes in the thorax: to this group belong the lympho-sarcomata. The point of origin of these growths is ordinarily in the deeper, and not in the peripheral portions of the chest. Cases are very rare in which the more external tissues serve as the starting-point. Lymphatic glands of various anatomical relations, as bronchial or mediastinal glands or tumors that have pressed their way downwards from the jugular glands may be first involved. A still larger category of growths arises from the pulmonary interstitial tissue—which in a measure acts in the lungs as Glisson's capsule does in the liver—surrounding and pursuing their course with the large vessels of the lungs: and in this way the peri-bronchial lympho-sarcoma has its origin. All these different examples have in common the resemblance in their structure to the lymphatic glands. What, however, distinguished these tumors most wonderfully, and of which fact pathology possessed few parallel examples, was the prolonged vitality of their elements, in consequence of which we find that they behave very much as if they were of the normal constituents of the body. They are almost always found as enclosed masses, and do not become caseous, and do not suppurate or ulcerate. In a small proportion of cases, a persisting thymus gives origin to a mediastinal lympho-sarcoma, occupying the same relative position as the thymus and occurring almost exclusively in young persons. Other subordinate forms, as chondromata, osteomata and to some extent teratomata, occur in the chest with rarity, but may attain to a not inconsiderable size.

Referring to the ulcerative forms of thoracic growths, the cancers stood at the front. These tumors have various points of origin within the thorax, independently of the metastatic and œsophageal varieties; they arise in pulmonary structure as well as in the glands above mentioned. The manner of dissemination of a pure bronchial cancer, there exhibited, was commented upon: The tumor had completely occluded the bronchus and terminated below in the shape of a polypus. A dissemination

thence took place into the smaller bronchi. These bronchial cancers belonged to the group of cancroids, were marked by strongly developed flat epithelium, and resembled the laryngeal and labial cancroid. Cancer of the lungs generally attains to considerable size before ulceration begins. He has been astonished to find that clinicians are generally agreed in the view that no characteristic cells of cancer could be found in the sputum. He was of the opinion that such cells can be found with a little more patience, for carcinoma of these localities have the peculiarity of casting off relatively large quantities of undecomposed material, so that by repeated examination the typical elements of the tumor can sooner or later be discovered in the sputum. Suppurative mediastinal tumors, having a syphilitic history, may also come up for differential diagnosis from the other forms already referred to; and these may have complications and sequels closely resembling the changes wrought, in surrounding structures, by tumors of the former classes. But as the report of the discussion in the *Medical Press* has it, VIRCHOW shows a reticence in regard to these conditions, for he feels that as to them "he is not a good giver of counsel." At the December meeting of the same Society, VIRCHOW returned to the subject, exhibiting a mediastinal cancer that had the macroscopic appearances of lympho-sarcoma. From its history, it was a metastatic growth, but there were no signs of retrograde change, and in the permanence of its tissues behaved exactly like a lympho-sarcoma—thus illustrating in a striking way the want of agreement sometimes observed between the microscopical and macroscopical appearances, and at the same time emphasizing VIRCHOW's teaching that patience and time are important elements where the diagnosis of tumors is concerned.

A CASE OF DOUBLE RIGHT KIDNEY, WITH NONE ON THE LEFT SIDE. PUNCTURE OF VEIN IN LAPAROTOMY. DEATH.

A case of misplacement of the kidney, reported as unique in character, was presented to the Academy of Medicine of New York, in its Section on Obstetrics, at its October meeting. DR. W. GILL WYLIE showed a kidney, abnormal in shape, which had during life been found beneath the right broad ligament. He stated that he had never heretofore seen a kidney situated so low down in the pelvis. There was another kidney on the right side, in its normal position, but there was no left organ. He was enabled to secure the misplaced organ a short time after it had been discovered; for the patient lost her life consequent upon surgical hæmorrhage, after an abdominal section. The case was interesting, furthermore, to the reporting surgeon and to his auditors, because it was the first death in a series of seventy consecutive laparotomies done by the reporter.

The operation passed along successfully enough, so far as could be seen, and the external wound was closed and dressed. But a small vein had been punctured with the needle in tying off the left broad ligament. This injured little vein had a low-lying position, and had not bled during the time that the ligament stump had been held up, but when it had been dropped back, it allowed a gradual venous hæmorrhage to become established, which ended fatally. If the surgeon had waited a little while after the operation, or if the earlier signs of blood-loss had been reported to him, so that he could have reopened the abdomen before the patient had been too far reduced, or if a drainage tube had been left in the wound for a little time, the fatal issue might have been avoided. As it was, the patient was too far gone to be saved by the second operation: she died the following morning. DR. WYLIE said that in the minds of some surgeons, "shock" was considered the *modus mortis* of cases which were, in point of fact, lost by hæmorrhage. Altogether too many cases have been reported as dying from shock. He would not deny the existence of shock in his practice, but it usually occurred while the case was on the operating table—in feeble women or in acute disease—and could be readily recognized at the time. The position of DR. WYLIE is that if the "shock" comes on during the second to the tenth hours after an abdominal section, and if at the same time there are the classical signs of internal hæmorrhage, the latter must form a demand for the immediate recall of the surgeon; no time is to be wasted in trying simply to stimulate the patient. The indications are to at once proceed surgically for the removal of the causes of blood-loss. This was done in the case referred to above, as soon as the surgeon was recalled to the case, but it was then too late to save the patient. Fuller details of the discussion of this interesting case are to be found in the *New York Journal of Gynecology*, for December, 1891.

SHOULD SYPHILITIC MEDICAL MEN CONTINUE IN PRACTICE?

DR. NEISSER, of Breslau, has considered the question of the expediency of the continuance in practice of physicians who have become syphilitic (*Centralblatt für Chirurgie*). His communication takes the form of a reply to a direct inquiry addressed to him by a professional colleague who had been advised both ways—to continue and to retire. NEISSER's conclusions are that the necessity for a physician to retire from practice must be the exception to the rule; provided, that he shall have been under an efficient specific treatment. He offers his views chiefly on the following conditions: First, concerning the stage of the disease; second, the thoroughness of the specific treatment down to the time when

practice is resumed; third, the state of the eruption, especially on the hands of the person whose line of practice is that of surgeon or accoucheur; fourth, whether any other affections of the skin, possibly not syphilitic in origin, may exist. The probabilities that a well-treated medical man will convey his disease to others are, of course, lessened in proportion to the remoteness of the date of his infection, and the lengthened interval since activity of efflorescence on skin or *mucosa* has been noticed; but even in recent cases, with popular eruptions and small ulcers, the writer holds that no serious danger need exist when the physician protects, as he should, the surfaces involved in the disease by means of rubber cots or impermeable dressings. In regard to non-syphilitic eruptions there is little probability of danger, where any ordinary degree of care is exercised; the eruptions themselves, NEISSER thinks, cannot be a source of infection, with the almost sole exception that blood might be conveyed from some abraded eruption to the raw surfaces on the patient. And with regard to this danger even, he does not consider that it has been settled. As to active engagement in obstetrical and surgical practice by a syphilized person, NEISSER claims that no hard-and-fast rule can be framed, and that very much must be left to the good judgment of the practitioner and to the merits of the case at the time the question of attendance shall be raised.

ANTISEPSIS FOR THE HANDS.

At the Johns Hopkins' Hospital the use of bichloride of mercury as an antiseptic has declined to a considerable extent in favor of solutions of permanganate of potash in combination with oxalic acid. DR. MALCOLM MCLEAN, at the October meeting of the New York Obstetrical Society, reported on his use of three formulæ, given below, for obtaining an aseptic condition of his hands (see *New York Journal of Gynecology* for December). Having briefly referred to the fact that DR. W. H. WELCH and other members of the Hopkins' surgical staff have come to the conclusion that corrosive sublimate solutions are inferior to those of the permanganate for many antiseptic purposes, the author says that he has found that the scrapings from the finger-nails, etc., taken after an ablution of the hands with any one of the ordinary antiseptic solutions, have developed, under culture in the laboratory, numerous germs. But when solutions of the permanganate of potash and oxalic acid had been used this was not the result, showing the superiority of the latter agents. The staining of the hands by the potash solution has been a serious objection, but he believes that this may be obviated by the use of a solution of hyposulphite of soda, one part to sixteen, and oxalic acid, one part to thirty-two of water. The steps of DR. MCLEAN'S process are (1), the hands, having been thoroughly cleaned, are to be held for two minutes in a solution of the permanganate of potash, five parts to one hundred, after which the hands should be rinsed in clear water; (2) hold the hands for one minute in a hypophosphite of soda solution, one ounce to the pint; and (3) while this is being done add the oxalic acid solution, one-half ounce to the pint of water. This causes a double chemical combination, whereby an oxalate of sodium and sulphur dioxide are formed, which have powerful decolorizing and disinfecting properties. The permanganate stains are promptly removed from skin and nails; after again rinsing the hands in sterilized water they are ready to come into contact with either an exposed serous or a lacerated mucous membrane. The hands may then be regarded as both surgically and obstetrically clean.

STATE CARE OF THE INSANE IN NEW YORK.

An investigating commission has been appointed in New York City to advise whether the city authorities shall continue to care for the insane or shall transfer them to State care. A recent census of the the insane inmates provided for by the city showed a total of 5,483; at Blackwell's Island, 1,813; on Hart's Island, 1,319; on Ward's Island, 1,919, and in the branch at Islip, 402. This number is being increased at the rate of two hundred, nearly, every year, which is somewhat less than the proportionate average annual growth of the city's population. The cost of maintenance of these insane persons is about thirty-six cents per diem. This is about forty per cent. less than the patients under the State's care cost. The aggregate cost to the taxpayers exceeds $500,000. But this does not relieve the city from being taxed in the general levy for the care of the insane throughout the State, so that the law offers to the county of New York a standing invitation, financial in character, to come into the fold of the State Lunacy Commission. The law passed in 1889 left it optional with three only of the counties, New York, Kings and Monroe, to retain their control of the treatment of the pauper lunatic, with the intent that those alleged "relics of barbarism," as they exist in the rural sections,—the county poor house and the county lunatic asylum—should by degrees be relieved from their long time responsibilities. A second object of the law was that the unfortunate lunatics should be placed under the medical care of superintendents who recognize that curability may be predicated of a certain proportion of unbalanced minds, even when they are classified as "chronic cases." The newly appointed commission consists of five members, no one of whom is a physician. The questions to be considered are, of course, primarily financial, but the commission will not be allowed to forget the interests of the pauper lunatic. The medical profession will see to it that

the advances already made in lunacy legislation shall not be put back. The poor lunatic has been the most sedulously robbed of all the classes of our communities. County officials and asylum trustees have had the almost unwatched control of the "crazy monies" and the least possible portions thereof that could be prevented found their way to the support and amelioration of the weak and defenceless pauper lunatic. Who was there in the community who could be plundered more readily? But now the times are different, and medical associations and other charity defense societies are organized and keenly alive to the perils of the poor, the insane and the unprotected. A great deal of volunteered watchfulness is being exercised as to the disposition sought to be made of charitable funds.

Surgery of Abdominal Wounds.—Dr. T. H. Manley contributes to the *Boston Medical and Surgical Journal*, October 8, a paragraph referring to the traumatic surgical work of Dr. Postempski at the Consolation Hospital, at Rome. He has had fourteen abdominal sections to perform in consequence of penetrating wounds of the peritoneum. In all these cases the wound was inflicted by a blade, mostly the stiletto. All of these patients recovered. The hospital in question is centrally located in the Piazza del Forum and in the institution which receives a very large share of the traumatic surgical cases of that city.

The Yale Students' Infirmary.—The ladies of New York City and vicinity have been organized for some months past on behalf of the Yale students, and their better care when sick. An infirmary of small size is their objective point, and the committee seek to raise $25,000. Subscriptions to the amount of $22,000 are said to have been promised on or before the last week in December, 1890.

A Memorial Hospital at Johnstown, Pa.—On February 4 the dedication of the Conemaugh Valley Memorial Hospital took place with suitable religious and historical addresses. Drs. Forbes and Pancoast were present from Philadelphia as representatives of the medical profession of a city that exerted itself nobly to mitigate the sufferings caused by one of the most disastrous calamities of modern times. After the ceremonial of presenting the new hospital to the Board of Citizen Trustees, an inspection of the building was made by the visitors. The cost of construction and furnishing has been defrayed by the State Flood Commission, and amounted to over $65,000. This expenditure forms a fitting close to the work of the Commission, which was entrusted with the disposal of not less than $3,750,000 for the relief of the desolated valley.

Incidents of the Influenza Epidemic.—Recent information from the Continent shows that Charcot and Billroth have, about the same time, been victims of the epidemic. At the time that Billroth was ill, there were four or five others, professors in the University, who were prevented by the same cause from going on with their lectures or clinics. The lunatic colony at Gheel has had many cases of *la grippe*, many deaths and many prostrating sequelæ.

DOMESTIC CORRESPONDENCE.

Public Health Department.

Wissinoming, Philadelphia, Pa., Feb. 1, 1892.

Hon. M. S. Quay, United States Senator:

My Dear Sir.—Permit me, as a member of the American Medical Association, to invite your special attention to the Bill introduced by Senator Sherman, providing for a National Bureau of Health, and the appointment of a Cabinet Secretary of State Medicine.

The importance of the measure can hardly be overestimated. You know what vaccination has done towards protection against small-pox (even for your own constituents in Philadelphia), and what quarantine does in protecting us against yellow fever and similar invasions. Now, sir, our profession, and especially members of the American Medical Association, stand prepared to prove that the dreadful disease, diphtheria, and many similarly destructive pestilences, *only exist* in our midst because concerted action, properly directed, cannot be *enforced*. With the appointment of a National Secretary, we could be assured of useful application of all the important information concerning epidemic diseases already acquired, while the system of thorough investigation of all such subjects will then, and *only* then, become mastered.

We are now like an army of good soldiers without a general. Very truly yours, N. Roe Bradner, M.D.

United States Senate, Washington, D. C., Feb. 5, 1892.

N. Roe Bradner, M.D., Wissinoming, Philadelphia, Pa.:

My Dear Sir.—Your letter of the 1st inst. is at hand. The proposition contemplated in Senator Sherman's bill—to form a National Board of Health—is, in my opinion, an excellent one, and I intend to support any legislation looking to its adoption. I am very truly yours, M. S. Quay.

To the Editor of the Journal of the American Medical Association:

The Adams County (Ills.) Medical Society, on February 8, presented the following testimonials to its senior member:

"Last week occurred the fiftieth anniversary of the doctorate of Dr. Francis Drude, an honored member and past-president of the society, he having graduated from the University of Berlin on the second day of February, 1842.

"When fifty years of active work in a profession so exacting and laborious and subject to so much risk and exposure as that of medicine still finds a man in the full possession of his mental vigor and retaining his early enthusiasm in scientific pursuits, the event is sufficiently rare and interesting to command especial notice.

"The Adams County Medical Society therefore gladly avails itself of the privilege of congratulating Dr. Drude upon reaching his golden anniversary, and to express and record the hope that it may yet for many years enjoy his presence, draw from the stores of wisdom garnered by his long experience, and catch new inspiration from his steadfast example."

From the society, Dr. Joseph Robbins then presented a beautifully engraved gold-mounted cane, and in an eloquent

address pledged anew the right-hand of fraternal fellowship to Dr. Drude.

That the Doctor fully appreciated the tokens thus bestowed was clearly evidenced, both in his grateful acceptance of them and in the hearty grasp of his good right hand. Respectfully yours, CHARLES W. ROOK.

SELECTIONS.

THE PAN-AMERICAN MEDICAL CONGRESS AND THE ROME MEETING.—The fact that the Pan-American and the International Congresses are to meet in the same year, and both of them in the autumn, has given rise to the impression among some that the former was conceived in opposition to the latter. Nothing, we are persuaded, can be more erroneous than this idea, and we believe that, so far from interfering, the Pan-American will actually help to increase the attendance at the Rome meeting. Many physicians from the Southern Republics, having already taken the long journey necessary to bring them to the Washington meeting and the World's Fair, will be prepared to extend their vacation and go to Rome, when, without this extra incentive, they might not have been persuaded to go to the International Congress.

The promoters of the Pan-American Congress were at pains to ascertain the date of the Rome meeting, so that there might be no interference, and with this object wrote to Dr. Baccelli several months ago. The latter replied that the Rome meeting would probably be held during the last week of September or the first week of October, although the exact time had not been definitely settled upon. This reply was not received until after the meeting of the Committee in St. Louis, when the date of the Pan-American had been already fixed for the first week in October; but when it was learned that this would interfere with the International, the time was changed to the first week in September. This will make it easy for those who desire to attend both meetings to do so, and is evidence that the Committee of the Pan-American Congress desire to promote rather than to antagonize the International. The Washington meeting must, of course, be held in 1893, in order to afford the opportunity to the Latin-American members of visiting the World's Fair, and the fact that that is also the year for the assembling of the International Congress is but a coincidence, which will, however, be of distinct advantage to both bodies.

The organization of the Pan-American Congress is in the hands of good men, the National Committee is a thoroughly representative one, and the delegates thus far selected from the other countries of the Continent are men of eminence at home, and many of them of an international reputation as well. The idea of an American Congress has been well received in our sister Republics, and, now that the danger of a hostile meeting with one of them has been happily averted, there is every reason to anticipate a most successful meeting, and one that will be productive of mutual advantage and pleasure to all who take part in it. And we are confident that the entire profession in this country will labor as a unit to aid the Commission in their good and patriotic efforts, and to further in every possible way the success of the first Pan-American Medical Congress.—*Medical Record.*

THE NATURE AND FREQUENCY OF DISEASE OF THE SPINAL CORD IN GENERAL PARALYSIS.—The spinal cord has been examined by Dr. Köberlin in twenty-three cases of general paralysis, examination being made of pieces from the cervical, dorsal and lumbar regions in each case, and the sections were stained with Weigert's hematoxylin and with carmine. Morbid change was seen in the lateral pyramidal tracts and posterior columns. Sometimes Goll's columns alone are degenerated, at others Burdach's columns are affected, though to a less extent in most instances. Cases were observed where Goll's columns were separated by streaks of degenerated tissue from the outlying area of Burdach's The morbid appearances comprise atrophy and degeneration of the medullated fibres, with excess of connective tissue, and the presence of numerous corpora amylacea. The degeneration was found to be remarkably symmetrical, both in transverse and longitudinal sections. In one case syringomyelia was found, but this case the author considers atypical. The cases were divided by Dr. Köberlin into: 1. Affection of the crossed pyramidal tract. 2. Affection of the posterior columns. 3. Affection of both. In one case of the first mentioned class, there was a difference in weight between the cerebral hemispheres, and as the more atrophied hemisphere was opposite to the diseased lateral tract, the author is disposed to think that the pyramidal degeneration was secondary. Amongst the cases in the second class, he gives reasons for considering the cord-disease as primary in some; in others, the disease was probably first developed in the brain. In none of the twenty-three cases were the anterior columns or the lateral cerebellar tracts diseased.—*Dublin Journal of Medical Science.*

A HIGHER PROFESSIONAL STANDARD.—Much has been said, of late, as to the necessity of elevating the standard of medical education. The work and results of medical colleges have been scrutinized and criticised, and laws passed and reformation advocated to raise the average college course, and regulate the routine of study.

All this is very well, and we are glad indeed that the best medical colleges in the land have responded to the demand. Our young men will be better fitted for their work, and the increased difficulty of entering the profession will prevent the old men from being crowded out by the great army of new-comers.

To stop short at the medical colleges, however, is far from right. When we see growing tendencies to loose and irregular practice among those who know better, and the strife and jealousy which so often exists among men whose education and work should make them brothers, we feel that there is an equal demand for a higher professional standard among those who are practicing medicine, as those who are at the threshold as students.

We have heard a man insist upon a higher grade course of study for students, and closer examination for graduates, while it was an open secret that his own methods were questionable and his personal character rotten. Indeed, it is often such fellows who make the most noise and, as a consequence, excite the most disgust.

Let the work of reformation which has begun in the schools, be carried on in the profession, until our ranks are free from those who disgrace themselves and all connected with them. There are enough of honest, right-minded men in the profession to freeze out or reform all these fellows—half doctors and half quacks. What we need is a recognition of a man's true standard, and the courage to treat him according to our convictions and his deserts.—*St. Louis Clinique.*

MIDWIFERY AMONG THE ALASKAN INDIANS.—The following notes on midwifery among the southeastern Alaskan Indians, the result of four years' experience amongst them, may be of interest: I will not discuss their superstitions, beliefs, and theories concerning midwifery, but will describe as briefly as possible their manner of conducting a case of labor.

When a woman arrives at full term a tent or hut is erected at some distance from the Indian village. A hole about two feet deep is dug in the ground inside the hut (or tent), and

is lined with moss. A stake is driven into the ground a few inches from the hole. When labor begins the woman is made to sit over the hole in a squatting position as though she were in the act of defecation and using the hole as a privy vault. She grasps the stake with both hands. One squaw sits behind with arms clasped around the patient's abdomen, and another sits at each knee to act as a brace or support. They make no examinations, the midwife not being allowed to see the vulva of the patient under any circumstances. When a pain comes the patient holds on to the stake, the midwife clasps her arms more firmly about the abdomen, and the other two women press firmly against the knees of the patient with their shoulders. This is repeated with every pain until labor is concluded. When the child is born it drops into the hole, where it is left for five or ten minutes, or sometimes longer. Sometimes the child is seriously injured by the fall, and bones are occasionally broken. In case of fracture no treatment is followed, the bone being allowed to unite as it will. The umbilical cord is divided about four inches from the navel. They do not cut it, but taking hold with their fingers partly twist it and partly pinch it off with their nails. They do not tie it, as the torsion prevents hæmorrhage.

The third stage is managed same as the second, the women supporting the patient and assisting her as before. The placenta is generally burned or cremated and the ashes preserved until the child dies. The Indians here cremate their dead, the ashes being afterwards put in a receptacle, which, with some of the property of the deceased, is placed in a small burial-house. With those ashes the ashes of the placenta are placed; sometimes, however, the placenta is buried.

After the placenta is removed, the binder is applied. This consists of a number of pieces of bark about a foot long and two or three inches wide, placed parallel to each other and quilted between two pieces of cloth or skin, like the bones of a pair of corsets. This, when applied, resembles a very coarse set of stays. The binder is left on ten days, during which time the woman is never washed. The blood and discharge become encrusted about the vulva and thighs, and, as it is moistened daily by the urine and lochia, her condition may be better imagined than described. Strange to say, I have never seen nor heard of a case of puerperal fever among them, although there have been about two hundred births in this neighborhood since I came here. During labor, and for ten days after, the woman is not allowed to eat or drink anything cold. Primiparæ remain in bed in the hut in which they were confined, for ten days, but many multiparæ get up and go about their work the first or second day.

To return to the child. It is first "fished" out of the hole and the cord divided. The midwife has a box or bladder containing a foul-smelling mass consisting of the leaves of some herb which has been chewed months before. A portion of this is applied to the stump and bound on. The child's face is then wiped, but no part of the body is washed. It is wrapped up and placed in a laced, bag-like arrangement (stiffened with bark in the same manner as the binder) which covers all of the child but the head. It is taken out of this to be cleaned three or four times daily. The child can be carried around like a sack of grain, but it is generally bound on the back of the mother. When the cord becomes detached it is covered by a piece of buckskin, which is nicely beaded. This is stitched to the breast of the child's clothing (like a rosette), and is worn by him until he is three or four years of age, when he is sent into the woods to hide it. There is a superstition connected with this which would take too long to detail.

In conclusion, I must say that their ignorance of midwifery is astonishing. In simple, uncomplicated cases they have, of course, no trouble; but if there is the slightest complication they are helpless.—J. K. Simpson, M.D., in *Occidental Medical Times.*

Inebriety.—In most cases inebriety is a self-limited disease. The drink symptom dies out naturally, or concentrates in some other form of morbid impulse. Any remedies or means used at the time of change will be credited as curative. The cessation of the drink impulse is not followed by full restoration, yet the impression prevails that total abstinence is a sign of cure always. Many pronounced paranoias and diseased persons who have abstained from alcohol, are posing as examples of cure from this or that means or remedy—persons in whom the drink impulse has died away naturally, no matter what remedy may be used. This is evident in the common class of those who sign the pledge, or profess conversion, many times, only to relapse after each occasion. Finally, in apparently the same circumstances, they go through the same formula, and the drink impulse disappears forever.

The real facts are that some organic brain change has taken place, the desire for alcohol ends. Other morbid symptoms may come on, but this disease has subsided or taken on new forms. The bark remedy, the mind cure, hypnotism, or any of the so-called specifics, that are followed by a cessation of a drink impulse, are all examples of this change. Physicians of asylums recognize this and direct all their efforts to build up and bring the patient back to a normal physiological life, in expectation of the final cessation of the drink symptom and restoration of the organic processes. This result may come on any time, and the object of all treatment is to encourage this, and remove the conditions which seem to provoke the drink symptom.

Drugs or restraint which holds the drink symptom in abeyance are never curative, and when followed by a subsidence of this impulse, it is an accidental conjunction of the natural dying away or change of brain function and growth. When such change occurs after long treatment in the best physiological and hygienic conditions, it is reasonable to suppose that these means have contributed more or less to this end. But when this subsidence follows in conditions opposed to this, and from means inadequate to change or alter organic action, clearly some other forces are at work.

The self-limitation of inebriety, and the natural history and progress of the disease are yet to be written.—*Quarterly Journal of Inebriety.*

Salicylate of Lithia.—Dr. Vulpian states that salicylate of lithia is more efficacious than salicylate of soda in cases of acute and progressive sub-acute articular rheumatism. It also has some effect in chronic cases when a certain number of the joints are still deformed, swollen, and painful.

Monobromide of Camphor for Spermatorrhœa.—The *Med. Summary* says: The monobromide of camphor has been successfully used in the treatment of spermatorrhœa, where a host of the usual remedies had been administered with no satisfactory results; finally the monobromide of camphor was given with prompt effect and perfect cures.

Salt-water (½ teaspoonful to the quart), by rectal injection, has been recommended in severe *acute anæmia*—for instance, from great loss of blood *intra partum.—Merck's Bulletin.*

Iodine-mixtures, according to Dr. Mann, are best prescribed with *molasses*, because the glucose contained in the latter not only disguises the taste of the iodine salts, but also protects them against chemical changes.—*Merck's Bulletin.*

NECROLOGY.

DR. FRANCIS J. METCALFE, of Florence, Italy, died in that city on the 9th inst. He was the son of that delightful clinical teacher, Dr. John T. Metcalfe, of Bellevue Hospital, New York. The subject of this note was born in 1850, graduated from the College of Physicians and Surgeons, New York, when he was 21 years old. The greater part of his life since then has been spent in Europe, in Florence especially, a most acceptable practitioner among the American residents there, and among tourists as well. He was a fine linguist.

MISCELLANY.

AN ARMY MEDICAL BOARD will be in session in New York City, N. Y., during April, 1892, for the examination of candidates for appointment in the Medical Corps of the United States Army, to fill existing vacancies.

Persons desiring to present themselves for examination by the board will make application to the Secretary of War, before April 1, 1892, for the necessary invitation, stating the date and place of birth, the place and State of permanent residence, the fact of American citizenship, the name of the medical college from whence they were graduated, and a record of service in hospital, if any, from the authorities thereof. The application should be accompanied by certificates based on personal knowledge, from at least two physicians of repute, as to professional standing, character, and moral habits. The candidate must be between 21 and 28 years of age, and a graduate from a Regular Medical College, as evidence of which, his diploma must be submitted to the board.

Further information regarding the examinations may be obtained by addressing the Surgeon General U. S. Army, Washington, D. C.

C. SUTHERLAND,
Surgeon General U. S. Army.

THE PAN-AMERICAN MEDICAL CONGRESS IN NEW YORK STATE.—At a meeting of the Medical Society of the State of New York at Albany, Feb. 5, a committee was appointed to coöperate in promoting the interests of the Pan-American Medical Congress. The committee consisted of Drs. A. Walter Suiter, A. Vanderveer, James D. Spencer, Seneca D. Powell, W. W. Potter, D. B. St. John Roosa, and John O. Roe.

MICHIGAN COMPLIMENTED.—The last bulletin of the Tennessee State Board of Health, January, 1892, speaking of Michigan, says: "This State, with Massachusetts, may justly claim a leading position in the public-health movement. The people support their State Board of Health"—which carries on the work—"under the long-continued guidance of one who is, perhaps, the most eminent sanitary authority in the Western world. A notable feature in the programme of this Board has long been the holding of sanitary conventions from time to time in different parts of the State, small towns not being overlooked." Speaking of a recent one: "The proceedings fill a closely-printed octavo pamphlet of forty-three pages, and are able papers and discussions of the points in which the parties requesting the convention are directly interested, read and conducted not mainly by the officers and members of the State Board of Health, but quite as much by intelligent and cultivated citizens of Negaunee. Michigan, the University State of the Northwest, is no less distinguished as a model for sanitary reform in all that vast region. Tennessee, the University State of the South, should not lag behind Michigan in sanitation. Especially should those cities and towns which are the seat of crowded institutions of learning, patronized by the people of many States, take the lead in this grand field of popular education."

A CONCOURSE will be held at Rush Medical College beginning Tuesday evening, March 1, for the purpose of filling the positions of Lecturer on Anatomy, and on Materia Medica, and Therapeutics in the Spring Faculty. The spring course begins March 1, directly after the close of the regular term, and continues two months with a class of from 250 to 300 students, thus affording the lecturers an excellent opportunity to exercise their skill as teachers. It is the policy of the college, so far as practicable, to fill vacancies in the regular faculty from the corps of spring instructors. Nine of the present members of the regular faculty have been selected in this way. The concourse will consist of twenty minute lectures by each of the applicants before the faculty, students and local profession upon subjects pertaining to their branches, which will be furnished by the Professors of Anatomy, and Materia Medica and Therapeutics, a week before the contest.

E. FLETCHER INGALS,
Registrar.

OFFICIAL LIST OF CHANGES in the Stations and Duties of Officers Serving in the Medical Department, U. S. Army, from February 6, 1892, to February 12, 1892.

Capt. Henry G. Burton, Asst. Surgeon, Capt. Arthur W. Taylor, Asst. Surgeon: The above named officers, having been found by Army Betiring Boards incapacitated for active service, on account of disability incident to the Service, are, by direction of the President, retired from active service, February 5, 1892, under the provision of section 1251, Revised Statutes.

OFFICIAL LIST OF CHANGES in the Medical Corps of the U. S. Navy, for the Week Ending February 13, 1892.

Asst. Surgeon P. H. Bryant, ordered to Naval Hospital, Philadelphia, Pa.

P. A. Surgeon H. T. Percy, detached from C. S. Str. "Patterson," and granted leave for two months.

P. A. Surgeon C. J. Decker, detached from Naval Hospital, Philadelphia, and to C. S. Str. "Patterson."

P. A. Surgeon John F. Uric, ordered to Naval Hospital, Portsmouth, N. H.

Surgeon Howard Wells, detached from Naval Hospital, Portsmouth, and wait orders.

Asst. Surgeon Joseph A. Guthrie, ordered to Naval Station, Port Royal, S. C.

Asst. Surgeon L. L. Young, detached from Naval Station, Port Royal, S. C., and to the receiving ship "Independence."

OFFICIAL LIST OF CHANGES of Stations and Duties of Medical Officers of the U. S. Marine-Hospital Service, for the Three Weeks Ending February 6, 1892.

Surgeon George Purviance, detailed as chairman, Board of Examiners. February 3, 1892.

Surgeon W. H. H. Hutton, detailed as member Board of Examiners. February 3, 1892.

Surgeon H. W. Sawtelle, granted leave of absence for ten days. January 30, 1892.

Surgeon Fairfax Irwin, granted leave of absence for fourteen days. January 26, 1892.

Surgeon F. W. Mead, detailed as recorder, Board of Examiners. February 3, 1892.

P. A. Surgeon H. R. Carter, granted leave of absence for seven days. January 20, 1892.

P. A. Surgeon D. A. Carmichael, when relieved, to proceed to Port Townsend, Wash., and assume command of Service. January 23, 1892.

P. A. Surgeon A. H. Glennan, when relieved, to proceed to South Atlantic Quarantine, and assume command of station. January 23, 1892.

P. A. Surgeon J. H. White, relieved from duty at South Atlantic Quarantine; to assume command of Service at Savannah, Ga. January 20, 1892.

P. A. Surgeon P. M. Carrington, when relieved, to proceed to Evansville, Ind., and assume command of the Service. January 20, 1892.

P. A. Surgeon G. M. Magruder, relieved from duty at New Orleans, La.; to assume command of Service at Portland, Ore. January 23, 1892.

Asst. Surgeon G. Vaughan, when relieved, to report to the Supervising Surgeon-General. January 20, 1892.

Asst. Surgeon J. O. Cobb, ordered to examination for promotion. February 3, 1892.

Asst. Surgeon J. B. Storer, ordered to examination for promotion. February 3, 1892.

Asst. Surgeon A. W. Condict, when relieved, to proceed to Wilmington, N. C., and assume command of the Service. January 23, 1892. Ordered to examination for promotion. February 3, 1892.

Asst. Surgeon C. H. Gardner, assigned to temporary duty at Baltimore, Md. January 27, 1892.

PROMOTIONS.

Surgeon H. R. Carter, commissioned by the President as Surgeon. January 28, 1892.

P. A. Surgeon G. T. Vaughan, commissioned by the President as P. A. Surgeon. February 6, 1892.

APPOINTMENT.

C. H. Gardner, of Maryland, commissioned by the President as Asst. Surgeon. January 28, 1892.